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CHANGES INDUCED
ON
ATMOSPHERIC AIR,
&c.

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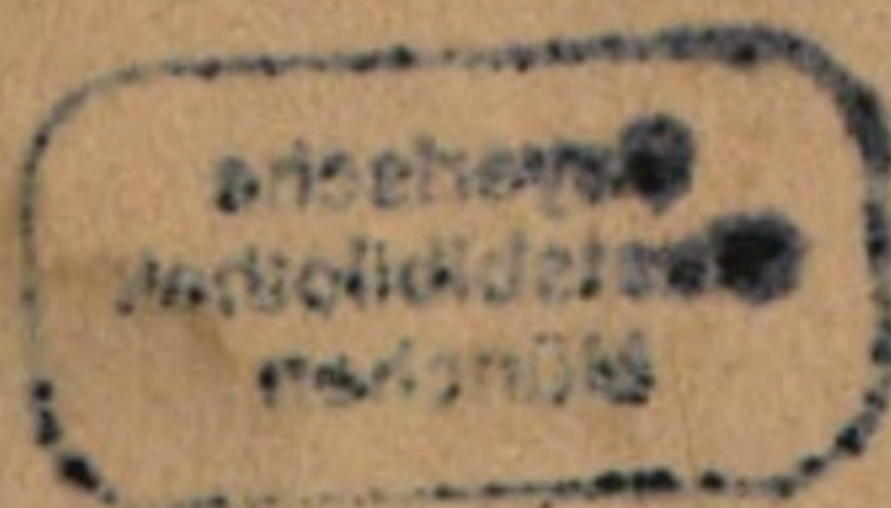
AN
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INTO
THE CHANGES INDUCED
ON
ATMOSPHERIC AIR,
BY
THE GERMINATION OF SEEDS,
THE VEGETATION OF PLANTS, AND
THE RESPIRATION OF ANIMALS.

By DANIEL ELLIS.

EDINBURGH :

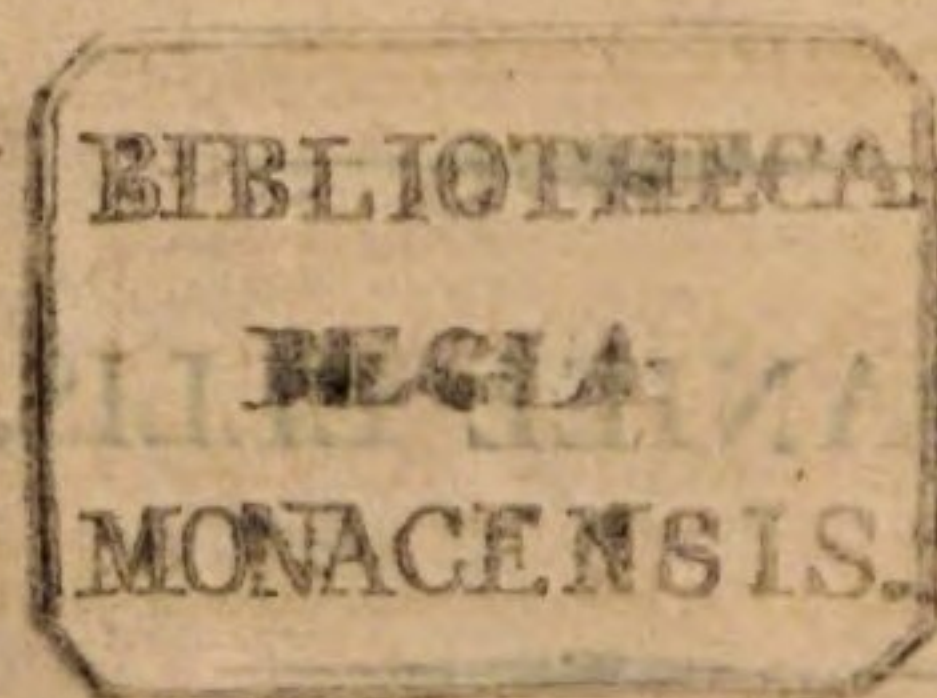
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TO
NICHOLAS NUGENT, M. D.

THIS TREATISE IS INSCRIBED,

IN TESTIMONY

OF RESPECT AND AFFECTION,

BY

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P R E F A C E.

THE investigation of the subject of the following Treatise was suggested to the author by accidentally observing the spontaneous recovery of an animal in whom all the appearances of life had been suspended by drowning. Reflecting on this part of the pathology of respiration, and on the theories which have been proposed to explain it, he was led to consider with particular attention the physiology of that function. The result of his inquiry terminated in a conviction, that although many great and important steps had been made, yet much hypothetical conjecture was blended with established fact, and many suppositions were admitted into our theories which but ill accorded with the structure and economy of the animal system.

By comparing attentively the chemical facts which relate to this function with certain pathological appearances, and considering both in connection with the actual structure of the respiratory organs, he was induced, not only to reject the sufficiency of the explanations which have been hitherto proposed, but to form some opinions on the subject more consonant, as it appeared to him, with the real designs of nature. In the course of this investigation, analogy readily suggested to him a comparison of the facts ascertained in human respiration with those which have been observed in the respiration of the inferior animals ; and, from the lowest order of animal beings, the transition to the analogous phenomena which occur in the vegetable kingdom, was natural and obvious. Thus, in a descending series, all the great classes of animated nature were successively brought under his review ; and, arriving ultimately at the most simple form of existence, he was led to make it the first subject of investigation, and then to retrace his steps through the more complex and perfect forms of vegetable and animal life.

It has been the constant aim of the author to make observation and experiment the basis of all his reasonings, and to deduce his conclusions from a full and distinct consideration of all the circumstances which seemed necessarily to affect them. The facts

which he has introduced in support of these conclusions have been collected from the best sources : and where they appeared to him to be either erroneous or defective, he has, in many cases, attempted to correct or supply them by experiments of his own. On every occasion of importance, where his results differed materially from those of preceding authors, he has endeavoured to compensate for the want of experience by a frequent repetition of his experiments : and, without regard to any particular opinions, he has detailed their results with all the accuracy in his power. These results, it is hoped, will, in general, authorise the conclusions which have been drawn from them ; but, he is well aware, that many points still remain which require farther elucidation, and are susceptible of more accurate proof. In the present state of science, however, the physiologist must often be content with probability, nor expect to attain to demonstration but by very gradual approaches to truth.

Much attention has likewise been bestowed to present the several facts, connected with the subject, in the order in which they have been successively made known. By thus blending with the recital of facts, the history of their gradual development, an additional degree of interest is imparted to them, and an opportunity is, at the same

time, afforded of doing justice to those eminent persons who have preceded him in this branch of inquiry. To those also who feel a pleasure in tracing the progress of the mind through the details of an extensive and complicated investigation, it may be interesting to observe, in what a slow and successive manner the knowledge which we at present possess has been acquired: how many individuals, often at very distant periods, have been employed in adding to it: and how many observations, apparently, at first, trivial and insulated, have, in the progress of science, assumed an unexpected importance, serving to connect together a long series of facts, which, but for such casual aid, might still have remained disjointed and broken. Reflections such as these, while they encourage every one to contribute his share, however small, to the general stock of knowledge, may, at the same time, serve to abate somewhat of that unpardonable vanity which has led some philosophers to assume the sole merit of having raised the edifice, when, in fact, the foundation was entirely laid by others, and they themselves have only assisted in arranging and giving form to the materials of which the superstructure may hereafter be composed.

Without entering into formal and minute description, it has been a principal object through the

whole of this inquiry, to combine anatomical fact with those reasonings which relate immediately to the living system. Unfortunately for physiology, this circumstance has not always been sufficiently kept in view, and the laws which govern the movements of inanimate matter have too frequently been applied, without reserve or discrimination, to explain the functions of organized and living beings. To a certain extent, indeed, animated bodies are subject to the same laws as all other material substances; but the peculiar properties which they derive from the principle of life can never be explained on mechanical or chemical principles. Although, therefore, it will be found, that chemical phenomena have received a due share of attention, yet they have at all times been held in subservience to the established truths of anatomy, and to those laws which peculiarly belong to bodies possessed of the principle of life.

In the present Treatise, no reference whatever has been made to the theories which have been proposed to explain the phenomena of vegetation and respiration. Such theories, it is evident, must rest entirely on a knowledge of the changes produced by living bodies on the air; and it is the professed object of this inquiry to examine, with more attention than has yet been done, into the na-

ture and extent of these changes. It will be proper, therefore, in the reader to abstract as much as possible from any preconceived opinions on these subjects; and to consider, that it is only by a scrupulous examination of the facts adduced, and of the legitimacy of the conclusions drawn from them, that the views of the author can be finally established or overthrown. To try the merits of these views, not by the evidence on which they rest, but by anticipating difficulties which they may be supposed hereafter to create, would be an open departure from the only mode in which we can ever hope faithfully to interpret the laws of nature.

Whatever may be the ultimate judgment passed upon the general merits of his performance, the author indulges the hope, that some advantage at least may indirectly result from it. The extensive series of facts which he has brought together, and the analogies which, from the evidence of experiment, he has endeavoured to trace among them, may direct the attention of future inquirers to a more comprehensive view of the subject than has yet been taken, and impart a new degree of interest and utility to the research. The attempt also to combine, in every instance, the demonstrations of anatomy with the chemical phenomena which we observe, and to consider both in connection and sub-

servience to the laws which characterise living beings, will, he trusts, meet with the approbation of physiologists; and tend to reduce within proper limits the application of chemistry to this science. At any rate, the present inquiry may serve to recall to a new and more accurate examination, the facts on which our knowledge of these subjects is founded: and in the present state of science, such an examination cannot fail to dispel much of the error and obscurity in which they are still involved.

EDINBURGH, }
MARCH 20. 1807. }

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It is to be regretted that the author has not been able to extend his researches into the details of the various subjects which are here treated, and that he has not been able to give a more complete and accurate account of the progress of the science.

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CHAP. I.

OF THE CHANGES INDUCED ON THE AIR, BY THE GERMINATION OF SEEDS.

1. **I**T is a well-known fact, that dried seeds, although exposed to heat and air, may be kept for a great length of time without undergoing any sensible change; nor do they in that state, as we found by experiment, in the least degree affect the air in contact with them. If, however, moisture have access to them, they presently begin to swell, which is the first step towards their evolution. This happens whether they be placed in water containing air, or in that which has been previously deprived of its air by long boiling; for equal numbers of the same peas, at the temperature of 60° Fahrenheit, being put into separate phials of fresh and of boiled water, and then corked and sealed, exhibited the same appearances, and made a like progress. In a few hours the seeds in both phials were much enlarged, and by the next day greatly swollen: the radicles of many

began to unfold, and they afterwards grew well, if duly exposed to the air; but when retained in this situation for a few days, their outer coats began to separate; the water in each phial emitted a putrid smell, and, on being added to lime-water, threw down a copious precipitate, after which germination did not take place. Mr Gough ascertained, that after five days steeping in water at 46° , and even in forty-eight hours, at temperatures from 60° to 66° , putrefaction in seeds came on, under which carbonic acid and carburetted hydrogen gases were produced, and the faculty of germinating was then destroyed*. M. Huber found also, that both in boiled and distilled water, a small degree of germination might be made to take place in peas; but during their submersion, the radicles never increased beyond three or four lines, after which they began to decompose, carbonic acid being first produced, and afterwards carburetted hydrogen gas. The whole substance of the pea, with the exception of its membranes, is, according to Saussure, reduced into these two gases†. It is evident from these facts, that the presence of water alone, is essential to the commencement of germination; and that if its application be too long and exclusively continued, it disposes to putrefaction.

2. Besides water, a certain degree of heat is necessary also to the germinating process; for no seed

* Manchester Memoirs, vol. iv.

† Mém. sur la Germination, par M. M. Huber et Sennebier, p. 128. 157.

can be made to grow at or below the freezing point, and above that point, the degree of growth will be always more or less influenced by that of temperature. Many seeds, however, will germinate as well as ever after having been frozen, or having been kept in frozen water. Although cold, therefore, does not destroy the germinating faculty of seeds, yet a certain degree of heat is essential to the display of it, and almost every species of seed seems to require a degree peculiar to itself; for each has its peculiar season of germination, which season varies with the temperature of the air.

3. Light is another agent which has been supposed to possess a considerable influence in the germination of seeds. The experiments of Hooke, Scheele, and Herschell, have established the distinction between the heating and illuminating powers of the rays of light: and a third portion of these rays has been supposed to enter more peculiarly into combination with various substances, and thence have acquired the denomination of the Chemical Rays. Dr Ingenhousz and M. Sennebier considered the presence of light to impede the growth of seeds; but the Abbé Bertholin affirmed, that the difference in the germination of seeds in the shade and in the light was owing not to the light itself, but to the greater evaporation of moisture which seeds exposed to the light suffered: and he added, that if the seeds in both cases were kept equally moist, those in the sun germinated sooner than those in the shade. M. Sennebier, however, repeated his experiments, and employed every precaution to ensure an equality of moisture in both situations, and he constantly found

the seeds in the shade to germinate sooner than those in the light *. The more equable warmth which seeds preserve, when excluded from the direct contact of light, may be a principal reason why they grow best in the dark ; and the evaporation which takes place on exposure, to which the Abbé alludes, would not only deprive the seed of the water essential to its growth, but be at the same time a considerable source of cold. In many trials we found, that steeped seeds, when exposed in the open air, rapidly lost their moisture, and made no effort to germinate ; but the same seeds, when confined in a glass-vessel, so as to check the progress of evaporation, germinated freely, though exposed to the same temperature, and the same degree of light. Hence we infer, that light, simply, has little influence either in promoting or retarding the germination of seeds.

4. But although water and heat appear to be the only agents essential to the beginning of germination, yet after a certain period air becomes equally necessary. Mr Boyle, Ray, and many other philosophers, ascertained that seeds would not grow in the *vacuum* of an air-pump. Some lettuce-seeds were sown upon earth in the open air, and some of the same seeds, at the same time, on another portion of earth placed in a glass-receiver, which was afterwards exhausted of air. The seeds exposed to the air grew nearly an inch and a half high in eight days ; but those in the exhausted receiver not at all. Air, however, being now admitted into the receiver,

* Thomson's System of Chemistry, vol. iv. p. 236. 1st Edit.

it was found, that in the space of one week the seeds had grown to the height of two or three inches *. Mr Gough observed, that steeped seeds put into a close vessel so as to fill it, did not grow, but that when a smaller number was put into the vessel, and its mouth covered loosely with a piece of glass, so that the air had free access to the seeds, they germinated very freely; experiments which we have often repeated, and always with the same results. Since, then, air is essential to the progress of germination, and it is known that this elastic fluid consists of two elementary parts, we may next inquire what changes this air undergoes, and whether one only, or both its parts, be required in this process.

5. It appears from the experiments of Mr Achard and others, that no seed will germinate in nitrogen gas, which gas forms nearly four-fifths of the air of the atmosphere. Mr Gough placed some barley-seeds, previously steeped in water, in a jar of this gas. At the end of three days they had suffered no change; the bulk of gas was not perceptibly altered, nor did it diminish by agitation in water, which showed that no carbonic acid was formed: but when the place of the nitrogen gas was supplied by atmospheric air, the same seeds germinated freely, and formed carbonic acid, just as if they had never been confined in nitrogen gas at all. If, however, they remained three or four days in this gas when the weather was warm, they began to putrefy in the same manner as when retained too long in

* Lowthorpe's Abrid. Phil. Trans. vol. ii. p. 206.

water, carbonic acid and carburetted hydrogen gases being formed, after which they could not be brought to germinate *. Mr Cruickshank introduced some soaked barley-seeds into a jar containing nitrogen gas, inverted over mercury. At the end of twelve or fourteen days there was not the least appearance of germination, but the gas had increased in bulk about one-fifth, containing from one-third to one-fourth of its bulk of carbonic acid; but neither the original nitrogen gas nor the barley had undergone any sensible change †. M. Huber found also, that a quantity of carbonic acid was produced when seeds were placed in a jar of nitrogen gas, and duly supplied with water; and that in a few days the radicle in a small degree was protruded, which he considers as a proof of germination ‡. But it appears that the same protrusion of the radicle takes place when seeds are submerged in water; and M. de Saussure, who repeated these experiments in pure nitrogen gas inverted over mercury, observes, that the radicle elongated only in a small degree, which is the consequence of the swelling of all the parts of the seed from the imbibition of water, and must not be considered as a true germination §. From these facts, therefore, it may be concluded, that although seeds, after being steeped in water, yield carbonic acid when confined in nitrogen gas, yet that they are in no respect affected by it; neither does that gas itself undergo any sensible alteration.

* Manchester Memoirs, vol. iv. p. 322.

† Experiments on Sugar, in Rollo on Diabetes, vol. ii. p. 218.

‡ Sur la Germination, p. 175. § Ibid. p. 184.

6. As thus the nitrogenous portion of the air seems neither to produce nor to suffer any change in the process of germination, let us next inquire into the changes which the oxygenous part, the other elementary portion of our atmosphere, undergoes. Mr Scheele and Mr Achard proved that oxygen gas was absolutely necessary for the germination of all seeds. Mr Gough likewise observed, that steeped barley-seeds, supported by means of a small hoop in an inverted jar of atmospheric air, germinated freely: the residual air, on the fifth day, being passed through lime-water, precipitated the lime, and, when thus freed from carbonic acid, was found to have lost nearly one-sixth of its original bulk, and the remaining air repeatedly extinguished a taper *. Some grains of barley, which had been soaked in water twenty-four hours, were introduced by Mr Cruickshank, on the 1st of December, into a jar of common air inverted over water, and preserved in a temperature between 60° and 70° . At the end of five days they began to grow, and on the 28th day, the greatest part of them had thrown out shoots half an inch in length. Germination continued to proceed on the 7th of February, when the barley being withdrawn, was found to be very sweet, and nearly converted into the state of malt. The air in the jar was found to consist only of nitrogen and carbonic acid gases, the whole of the oxygen having entirely disappeared. When the experiment was made in a jar containing forty-six measures of very pure oxygen gas inverted over mercury, the process of ger-

* Manchester Memoirs, vol. iv. p. 319.

mination was considerably hastened : the column of gas on the 10th day had suffered no apparent change ; and the residual air then consisted of carbonic acid, mixed with only 1-50th of its bulk of oxygen. The barley was partly converted into malt*. These experiments teach us, that oxygen gas is essential to the process of germination ; that it gradually and completely disappears during the continuance of that process ; and that a large quantity of carbonic acid supplies its place.

7. M. de Saussure *junior*, instituted some experiments to determine the proportion which the carbonic acid, produced in germination, bore to that of the oxygen gas, which disappeared in that process. He conveyed eighteen peas into a glass-vessel, the mouth of which was plunged in mercury, and which contained 11.5 cubic inches of atmospheric air, that had been previously well washed in lime-water. About one-fourth of a cubic inch of water was then passed into the jar, and floated on the mercury, so that the peas were about half immersed in it. In ten days the radicles had sprouted about one-third of an inch ; and the air in the jar, after making the necessary corrections for pressure and temperature, had undergone no sensible diminution of volume. The residual air being submitted to analysis, lost $\frac{9}{100}$ by agitation with lime-water, and the remainder afterwards suffered a further loss of $\frac{12}{100}$ by the eudiometrical test of phosphorus. If the air he employed be held to contain $\frac{21}{100}$ of oxygen gas, (which

* Experiments on Sugar, p. 213. 215.

that of his laboratory was found to do), the 11.5 cubic inches of it which he used in this experiment, will, he says, consist, before the introduction of the peas, of 3.105 of oxygen, and 8.395 of nitrogen: and at the close of the experiment he states the composition of the air at 1.88 oxygen, 8.395 nitrogen, and 1.035 carbonic acid; consequently 1.255 of oxygen were employed to form 1.035 cubic inches of carbonic acid, a result as near to truth as can be expected in observations on small volumes of air. In other experiments made over water, nearly the same results are said to have been obtained*. But some error seems to have crept into the calculations of M. de Saussure; for, if with him we consider the atmosphere as containing $\frac{21}{100}$ of oxygen gas, 11.5 cubic inches of air will contain 2.415 of oxygen, and 9.085 of nitrogen; proportions very different from those which he has assigned, and which necessarily affect all the subsequent calculations, and the conclusions he has drawn from them.

8. With the view, therefore, of ascertaining this point with more precision, we placed one dozen of peas, which had been steeped in water thirty-six hours, and whose surfaces were dried afterwards by blotting paper, on a small whalebone hoop covered with gauze, which was pushed half way up into a glass-jar well dried, and of the capacity of 18.2 cubic inches. This jar, filled with atmospheric air, was then inverted into a deep saucer, where it stood over a small glass-cup, containing half a cubic inch

* Journal de Physique, tom. xlix. p. 92.

of the water of potassa, which is well known to have a strong affinity for carbonic acid. A quantity of mercury was now poured into the saucer, and the jar was kept steady by a weight laid upon it. The whole was then set aside in a room, where the barometer stood at 29.3 inches, and the thermometer at 62.5° . By the following day, the peas had sprouted about 1-4th of an inch, the mercury in the saucer had risen 3-10ths of an inch into the jar; and as the small cup with its solution floated on the mercury, it was necessarily raised into the jar with it. By the end of the second day, the radicles were 3-4ths of an inch long, and the mercury had risen nearly 7-10ths of an inch into the jar, as appeared by a small paper scale, graduated to inches and tenths, and pasted on the outside of the jar. At the close of the third day, the radicles were to appearance about an inch long, and the mercury in the jar was nearly an inch in height. And by the fourth day, the radicles had made little additional progress, but the mercury stood nearly 1.1 inch in the jar. During the following day, no apparent changes occurred, and the mercury in the jar was now exactly 1.1 inch in height, when that in the saucer was brought to a level with it. The barometer at this period was 29.95, and the thermometer 65° .

9. Having by other experiments ascertained, that when the mercury had reached this elevation, and became stationary, the oxygen gas of the air employed had completely disappeared, we proceeded to determine the proportion which this diminution in the bulk of air, evinced by the rising of the mercury, bore to that of the oxygen gas which the air

originally contained. The jar, therefore, was cautiously raised out of the mercury, and the attraction of carbonic acid by the alkaline solution was proved by the brisk effervescence which the acetous and diluted sulphuric acids excited in it. The glass-cup, containing a bulk of water equal to that of the solution employed, was now again put into the jar, previously replaced on its bottom, together with the hoop and peas. Water was next poured in till it reached that part of the jar to which the mercury had risen, and to raise it thereto required 12.5 cubic inches, which, therefore, was the bulk of residual air at the close of the experiment; and afterwards, to fill the jar completely, it required four cubic inches more, which consequently was the measure of the volume of air that had disappeared. But these two quantities amount only to 16.5, and the capacity of the jar has been stated to be 18.2 cubic inches: the difference, therefore, equal to 1.7, will be the space which the several substances occupied within the jar, and which reduces the actual volume of air employed in the experiment to 16.5 cubic inches, of which a portion equal to four, or $\frac{1}{4.125}$ of the whole, had disappeared.

10. But the volumes of elastic fluids are inversely as the weights by which they are compressed; and for every degree of Fahrenheit's thermometer, Mr Dalton has found that atmospheric air is dilated $\frac{1}{483}$ part of its bulk under common atmospheric pressure*. Making therefore, on these accounts, the

* Manchester Memoirs, vol. v.

necessary reductions, we have $\frac{29.95 \times 12.5}{29.3} = 12.777$;
 but $\frac{2.5 \times 12.777}{483} = .0661$ and $12.777 - .0661 =$
 12.7109 , which is the actual volume of air at the
 end of the experiment when reduced to the same
 pressure and temperature as it possessed at the be-
 ginning. To find, farther, the proportion which
 this loss of bulk bears to that of the whole air em-
 ployed, we have $16.5 - 12.7109 = 3.7891$ and
 $\frac{3.7891}{16.5} = \frac{1}{4.354}$. But atmospheric air contains $\frac{22}{100} = \frac{1}{4.54}$
 of oxygen gas, so that the loss in the bulk of air by
 this experiment is rather greater than the propor-
 tional quantity of oxygen gas which the atmosphere
 contains. In another trial, made in the same jar,
 and under similar circumstances, the loss of bulk
 which the air was found to have suffered, after the
 necessary reductions were made, was $\frac{1}{4.43}$ of the
 whole. Hence, therefore, if it be granted that the
 loss of bulk which the air suffered arose from the
 attraction of the carbonic acid by the water of pot-
 assa ; and if it be also allowed, that all the oxygen
 gas of the air employed had completely disappeared,
 it must be concluded, that the bulk of carbonic acid
 produced nearly corresponded to that of the oxy-
 gen gas lost. This, too, will appear more directly,
 by comparing the diminution which a given bulk of
 air suffers in germination, with the proportion of
 oxygen gas which the atmosphere contains : Thus,
 $100 : 22 :: 16.5 : 3.63$, which comes near to 3.78,
 the actual loss of bulk which the air in the foregoing
 experiment underwent.

11. That the whole loss of bulk which the air suffered in the foregoing experiments, did actually arise from the formation of carbonic acid, and its subsequent attraction by the alkaline solution, appears almost certain, from the opposite results obtained when no such solution was employed. In the experiment of Mr Cruickshank, (6.), made in pure oxygen gas, the original volume of gas suffered neither increase nor diminution, although it was almost entirely converted into carbonic acid; and the same observation was made in all the experiments of Saussure. In a great number of experiments also, we observed, that where no water of potassa was placed within the jar, the seeds equally grew; the oxygen gas was completely destroyed, and the residual air lost about 1-5th of its bulk by agitation in lime-water, although the volume of air was only in a small degree diminished. This trifling diminution was probably owing to the necessary condensation which oxygen gas experiences when it combines with carbon, and which, according to Crawford and Guyton, amounts to about 1-7th of the bulk of gas employed *, although it is probable that this estimate is considerably overrated.

12. We endeavoured to ascertain the amount of this diminution in germination, by causing some soaked peas to grow, supported on a whalebone hoop, in a small flask with a narrow neck, which was inverted into an ale-glass containing mercury. When the peas had ceased to grow, the rise of the

* Murray's System of Chemistry, vol. ii. p. 352.

mercury into the neck of the flask was accurately marked, and the air was then analyzed. The destruction of its oxygen gas was found to be complete, and the usual quantity of carbonic acid to be formed. After deducting the space occupied by the seeds and the hoop which supported them, the volume of air, at the close of the experiment, was compared with that which it possessed at the beginning, and the difference noted. An estimate was then made of the quantity of oxygen gas originally contained in the air employed, and the loss of bulk was considered to arise from the condensation attending its conversion into carbonic acid, due attention being paid, at the same time, to corrections for pressure and temperature. The average amount of the diminution varied from 1-8th to 1-12th; or the volume of carbonic acid produced was about 1-10th less than that of oxygen gas which had disappeared. In some trials the amount of the diminution was much greater, but this was found to depend on the quantity of moisture given out by the seeds, and the more or less vaporific state in which it existed, and which source of error can be obviated only by reducing the air to the same state with regard to moisture at the end, as at the commencement of the experiment. From the whole, however, we may conclude, that a certain condensation attends the conversion of oxygen gas into carbonic acid in the process of germination, as well as in combustion; and consequently that a part of the diminution which the air suffers, is to be attributed to this cause. If this were not the case, the bulk of carbonic acid produced would appear greater than that of

oxygen gas lost, which the experiments both of Cruickshank and Saussure forbid us to believe, and which, in all our own experiments, appeared to be somewhat less.

13. If, then, we may be permitted to suppose, that in the process of germination the oxygen gas of the air loses about 1-10th of its bulk by passing into carbonic acid, it follows, that the 3.78 cubic inches of this acid, which appeared to be attracted in the foregoing experiment, (10.), will actually consist only of 3.402 cubic inches. Now, one cubic inch of carbonic acid weighs 0.467 of a grain, and therefore 3.402 cubic inches will weigh 1.589 grains, which may be taken as the weight of the carbonic acid actually produced. But farther, a cubic inch of oxygen gas weighs 0.3474 of a grain; and therefore 3.63 cubic inches (the bulk of that gas present in the experiment) will weigh 1.261 grains, which is less by 0.328 of a grain than the weight of the carbonic acid produced. This greater weight of the acid, therefore, must be attributed to the carbon which united with the oxygen gas to form it, and which makes the carbon to constitute $\frac{1}{4.84}$ of the compound. According to Lavoisier, carbonic acid contains $\frac{28}{100} = \frac{1}{3.57}$ of carbon; and according to Dr Priestley, the weight of the carbon is about 1-4th of the compound *: but Guyton, who formed this acid directly by the combustion of diamond, found that one part of diamond combined

* Experiments on Air Abridged, vol. iii. p. 377.

with a little more than four of oxygen, and consequently that the proportion of carbon was only 1-5th of the compound *. Hence, therefore, if these experiments and calculations be correct, the quantity of carbon, which enters into the carbonic acid formed by germination, is rather greater than the experiments of Guyton assign as the proportion of that substance which forms this acid in combustion.

14. M. Huber made experiments with the view of ascertaining the composition of the atmosphere best suited to carry on the germinating process. He found, that an artificial atmosphere, composed of the same constituent parts as common air, favoured it precisely in the same manner; that germination was retarded in proportion as the nitrogen gas exceeded the usual quantity of that gas contained in atmospheric air, but was little affected when the variation was small. A superabundance of oxygen gas also, although not destructive to germination, nevertheless retarded the process; for seeds were found to germinate better in a mixture formed of three parts nitrogen and one of oxygen, than in one composed of three of oxygen mixed with one of nitrogen †. Hydrogen gas, in the course of these experiments, was often substituted for nitrogen, and answered precisely the same uses, germination being more vigorous in proportion as the hydrogen came near to that of the nitrogen existing in the at-

* Annales de Chimie, tom. xxxi. p. 108.

† Sur la Germination, p. 31. et seq.

mosphere ; but neither in pure hydrogen nor nitrogen did the process at all go on *.

15. M. Huber proceeded next to ascertain the smallest quantity of oxygen that would suffice for germination. He found, that seeds did not grow in very small volumes of air ; and that the greater the number of seeds confined in a given volume of air, the more was their growth retarded ; that where the oxygen gas constituted only one-eighth of the atmosphere, no effect was produced on lettuce-seeds, but that one-sixth of it began to promote their growth. Lefevre, however, is said to have found, that an atmosphere containing only $\frac{1}{32}$ part of oxygen, favoured the evolution of raddish-seeds, which renders it probable that different seeds vary much in the proportion of oxygen necessary to carry on their germination †. When the atmosphere was impregnated with the vapour of sulphuric ether, of camphor, spirits of turpentine, assa-fœtida, vinegar and ammonia, the process of germination was more or less retarded. From the facts stated in this and the preceding paragraph, we learn, not only that air is necessary to the growth of seeds, but that the ordinary state of its composition in our atmosphere is, as M. Sennebier observes, the best adapted to produce a vigorous germination. This state, however, is not determined with such rigour that a small variation in the proportions or qualities essentially injure the process : it admits, on the contrary, of great latitude without much disturbance, which provision

* Sur la Germination, p. 58.

† Ibid. p. 64. et seq.

affords a correction for those occasional variations which our atmosphere experiences, and insures the preservation of vegetables in all circumstances in which they may naturally be placed *.

16. Such being the principal facts in germination which bear a relation to the changes which the air suffers, let us next inquire in what manner they are to be accounted for or explained ; and first of all, what becomes of the oxygen gas that disappears ? Mr Gough believes that seeds, during their germination, *absorb* oxygen gas from the air, retain a part of it, and reject the remainder, charged with carbon †. Now although seeds have a power of imbibing and exhaling water, there is no evidence of their possessing a structure fitted to absorb and expel aëriform fluids ; neither is there any proof of such fluids at any time existing in them. If the air be absorbed in its entire form, some receptacles ought to be shown in the seed for retaining and decomposing it, and from which the nitrogen gas could be afterwards expelled : or, if the oxygen gas alone be conceived to enter, then it may be asked, by what power of *absorption* it could be separated from the nitrogen gas with which it was previously combined ? Mr Cruickshank inclines also to the belief, that the oxygen gas is chiefly absorbed by the seed, although part may, he says, be consumed in the formation of the carbonic acid ‡ ; and we learn from his experi-

* Sur la Germination, p. 119. et seq.

† Manchester Memoirs, vol. iv. p. 319.

‡ Experiments on Sugar, in Rollo on Diabetes.

ments, (6.), that when seeds are made to grow in oxygen gas, the bulk of carbonic acid produced is equal to that of the oxygen gas which disappears. Even granting, therefore, that a portion of this gas is absorbed by the seed, this equality of proportion in the carbonic acid produced forbids us to consider any part of it to be retained; and if it be admitted, that the formation of this acid is in part effected without a previous absorption of oxygen, why should we not allow it to proceed to the fullest extent in which it takes place, rather than have recourse to two such opposite suppositions to account for the same phenomenon?

17. To suppose this oxygen gas to be taken up by the seed by the operation of chemical affinity, necessarily implies its previous separation from the nitrogen gas with which it was united; but how could this be done, unless the seed presented something to the air which had a stronger affinity for its oxygen than the nitrogenous portion has? And what could it offer but moisture and carbon? Moisture, however, does not decompose air; and if carbon be the agent, must not carbonic acid be at once formed? And if this acid be thus formed, exterior to the seed, and out of the oxygen gas in contact with it, how can we hold that gas to be first singly taken in by the seed, and expelled afterwards in the form of carbonic acid? To say that the air is attracted in its undecomposed state, necessarily requires proof of the existence of certain cavities in the seed where it can be retained; for as the nitrogen gas neither suffers (5.) nor produces change, it must be completely expelled after the oxygen is abstracted from

it. Lastly, M. de Saussure has endeavoured to show, that the carbonic acid formed in germination contains in it precisely the quantity of oxygen gas that has disappeared : And although, from the difference of opinion which prevails concerning the actual proportions of the elements which constitute that substance, this cannot be positively assumed ; yet the near proportions which, in our own experiments, as well as in those of Saussure and Cruickshank, the two gases bear to each other at the beginning and end of the process, renders it extremely probable. If this opinion be well founded, no part of the oxygen can be retained by the seed ; and we may conclude, therefore, with M. de Saussure, that none of it is either attracted or absorbed *.

18. If indeed oxygen gas were in part retained by seeds, we might expect, since they deteriorate so large a portion of it, that they would acquire weight as well as other substances with which the base of that gas combines. But Mr Gough found, that steeped peas germinating in air, and sprouting to the length even of two inches, did not increase in weight †. We found, that if peas be steeped a sufficient time in water, they almost exactly double their weight, which increase of weight rapidly diminishes if they be afterwards exposed freely to the air. Six peas, weighing nineteen grains, were, after forty-eight hours steeping in water, increased in weight to thirty-eight grains, and in this state were

* Journal de Physique, loc. cit.

† Manchester Memoirs, vol. iv. p. 316.

exposed on a table in a room at temperature 66° : they gradually decreased in weight, and by the fifth day had fallen back to their original weight and appearance. If, however, the direct effects of evaporation be prevented, by inclosing them in a jar, they still exhale moisture and diminish in weight, but much more slowly; for the peas in the foregoing experiment (8.) were by their germination much diminished in bulk, and lost rather more than seven grains in weight;—from all which it follows, that no supposed absorption of oxygen adds, during their growth, to the weight of seeds.

19. If then there be no proof that the oxygen gas which disappears in germination enters, either by absorption or by chemical affinity, into the seed, so as to combine with it, it follows, that this gas must be at once converted into the carbonic acid produced in that process, without such previous combination; and the formation of the acid proceeding always in proportion (7.8.) to the disappearance of the oxygen gas, sufficiently shows that it is derived from that source alone. If the acid were formed independent of the oxygen gas employed, the whole bulk of air ought to be increased by germination: and accordingly, Mr Cruickshank found, that when carbonic acid was produced by steeped seeds, confined either in nitrogen (5.) or hydrogen gas, the bulk of air was increased one-fifth, but nothing like germination then took place. He even found that seeds, after being soaked in water, and passed up into a tube of mercury, formed carbonic acid in large quantity, but without undergoing any sensible change in their ap-

pearance *. This experiment we repeated by passing up half a dozen soaked peas into a graduated tube filled with mercury, and immersed in a jar of the same fluid. By the following day two-tenths of a cubic inch of air were collected in the upper part of the tube, and the experiment was continued till the bulk of air was increased to 1.6 cubic inch, beyond which it did not proceed. Lime water was then passed into the tube, and with the aid of a little agitation, all the air except a small bubble was rapidly attracted by it. The peas did not exhibit the slightest sign of germination. These facts show, that the production of this acid during the growth of seeds is altogether different, both in its formation and the consequences which attend it, from that production of it which takes place where no living action exists. Where this acid is formed independent of germination, no change is effected in the seed, and the bulk of air is increased : where it is produced by that process, a part of the air disappears ; the evolution of the seed goes on ; and the quantity of the air is somewhat diminished. Hence, therefore, we conclude with De Saussure, that in germination the seed does not form carbonic acid from its own substance, but furnishes only one of the constituent parts of it, namely, the carbon † ; and farther, that when it does form this acid, independent of oxygen gas, it is only under a state of decomposition, (5.),

* Experiments on Sugar, loc. cit.

† Journal de Physique, tom. xlv. p. 98.

or in circumstances where no living action is going on.

20. As therefore the carbonic acid met with in germination is not emitted ready formed by the seed, it must be produced exterior to it; and for this purpose carbon must be in some way furnished. Dr Woodhouse supposes this carbon to proceed either from the earth in which the seeds are planted, from some decayed portion of the living leaves, or from the cotyledons of the seed itself*; but this acid has been shown to be formed where no earth is present, and before any leaves appear, (7. 8.), and where the seed itself exhibits no sign of decay. It is plain, therefore, that the living seed, during its germination, must possess a power of supplying carbon; and by the union of this carbon with the oxygen gas of the air, the carbonic acid that is met with must be formed. We find accordingly, that the carbonic acid produced does actually exceed in weight (13.) the oxygen gas that disappears; and the carbon that occasions this excess can be derived only from the living seed, which, if the experiments of Hassenfratz are to be trusted, contains less carbon after germination than it did before it was submitted to that process†.

21. But it has been said, that when the air used in this process contains no oxygen gas, or when all which it contained has been converted into carbonic acid, germination no longer goes on. This can-

* Nicholson's Journal, July 1802, p. 151.

† Annales de Chimie, tom. xiii. p. 188.

not arise from the presence of nitrogen gas, for that gas exerts (5.) no injurious operation. Neither, although Mr Achard found, that germination did not take place in carbonic acid, can the presence of that substance be considered as positively destructive to the process ; for the same effect occurs when nitrogen gas only is present, and even *in vacuo* without the direct agency of any gas at all. M. Huber too found, that when oxygen gas or common air, in certain proportions, was mixed with carbonic acid, seeds grew in it very well * : and in all our experiments, where the carbonic acid produced by germination was suffered to remain, the seeds ceased to grow, not from the presence or abundance of that acid, but from the diminished quantity or total absence of the oxygen gas out of which it was formed. Since, indeed, the absence of that gas is sufficient to account for the cessation of the process, it is unphilosophical, because unnecessary, to resort to the agency of any secondary cause ; and as both carbonic acid and nitrogen gas must always of necessity be present in germination, it is not to be expected that either the one or the other should exert any injurious operation.

22. Neither, because carbonic acid is always present, is it, on that account, to be held as at all essential to germination ; for the same might be urged in favour of nitrogen gas, which neither produces (5.) nor suffers change. The constant existence of this acid in the atmosphere is no proof of its aiding ger-

* Sur la Germination, p. 56.

mination. By many its proportion has been rated at $\frac{1}{100}$; but Berthollet considers this estimate as much too high *, and Dalton makes it amount only to $\frac{1}{1000}$ †,—a quantity much too inconsiderable to exert any sensible effect. When also M. de Saussure abstracted the carbonic acid from the air, previous to placing the seeds (7.) in it, germination went on as well as where this had not been done; and, on the other hand, this process continued to proceed when the acid was removed, as soon as formed (8.) by an alkaline solution, as when it was suffered to remain. It has been said, however, that when it is abstracted, by means of lime, from the air in which seeds are growing, the progress of their growth is retarded. The lime, indeed, does abstract the carbonic acid in the same manner as we have seen the alkaline solution to do: and, *à priori*, there seems no reason why its abstraction should in one case be more prejudicial than in the other. But this abstraction of the acid by the lime is carried on only after it has imbibed water; and we found, that when a quantity of powdered lime was placed underneath some peas growing in a jar inverted over mercury, it presently began to swell, the jar continued quite dry, and the peas germinated very imperfectly, being reduced nearly to half their bulk. Thus, they were deprived too rapidly of their moisture to enable them to grow, in the same manner as when they are too freely exposed (18.) to the air. In

* Chemical Statics, vol. i. p. 375.

† Manchester Memoirs, vol. i. new series.

fact, since carbonic acid is necessarily a product and consequence of germination, it seems absurd to consider it at the same time as an exciting principle and a cause.

CHAP. II.

OF THE CHANGES INDUCED ON THE AIR BY THE VEGETATION OF PLANTS.

23. **I**N the former chapter it was shewn, that water was a first and essential requisite to the germination of the seed, and numerous facts prove it to be equally necessary to the future progress of the plant ; for if completely deprived of water, the plant withers and dies, although every other requisite be present ; nor does it afterwards revive, even although water be supplied. The quantity of this necessary fluid that is daily absorbed by plants, varies greatly according to their structure and habits, to the temperature of the season, the supply of water, and the state in which vegetation is going on. After its absorption, it is again given out in great part by transpiration ; and the degree in which this latter process proceeds, must depend on the quantity previously absorbed : and conversely, whatever restrains the freedom of transpiration, must abridge

the power of absorption ; for when the vessels are once filled with fluid, if none be carried off, no more can enter. The absorption is carried on chiefly by the extremities of the roots, and the transpiration by the surfaces of the leaves. Dr Hales found, that a sunflower, three and a half feet high, lost by perspiration, on an average, 1 lb. 14 oz. of fluid *per* day. During a warm night, and without dew, it lost only 3 oz. : when the dew was sensible, it suffered no perceptible loss ; and if the dew was great, or rain fell, it gained a few ounces in weight. The surface of all the leaves of this plant was equal to about 39 square feet, and that of its roots to 15.8 square feet. The depth of water absorbed by the whole surface of the roots was $\frac{1}{67}$ part of an inch, and the depth perspired by the surface above ground $\frac{1}{165}$ part. He found likewise, that different plants varied greatly in the quantity of fluid which they perspired ; that evergreens perspired much less than other plants ; and that the perspiration was proportional to the extent of surface of the leaves, and ceased nearly if the leaves were removed*. The experiments of M. Guichard shew, that it is chiefly by the upper surfaces of the leaves that this transpiration of fluid is carried on ; and those of the same author, of Du Hamel and Bonnet, demonstrate likewise that absorption, as far as the leaves are concerned, is performed by their under surface. To prove this, the superior surface of one leaf and the inferior surface of another were covered with var-

* Statical Essays, vol. i. pp. 4. 6. 21. & 28.

nish, and the former, in a given time, suffered little diminution of weight, but the latter became much lighter. Similar leaves also, were laid on the surface of water, and those which had their superior surface inverted gained little weight, and for the most part died in a few days; while such as had their inferior surface applied to the water, became much heavier, and flourished many months. These facts make it evident, that perspiration and absorption are not performed by the same vessels, but that each has its peculiar organs *. The office of absorption is performed by fine hairs or points, placed on the under surface of the leaves, which in many plants are hollow tubes constructed for that purpose. When leaves have no such points, small apertures are found in their place †. The leaves of plants become gradually less and less fit for carrying on these functions; not so much, however, from any decay or inaptitude in their structure, as from the declining influence of another agent, whose operation we are next to consider.

24. The fluids of plants which are taken up by the roots, and afterwards perspired by the leaves, require, for their due transmission through the stem, a certain degree of heat: hence they flow most freely in the day and in sunshine, and are checked during the night and in frost. Dr Hales observes, that during moisture and warmth, the flow of the sap is most vigorous, but that a cold easterly wind imme-

* Dr Bell, Manchester Memoirs, vol. ii. p. 411.

† Wildenow's Principles of Botany, p. 298.

diately abates it. When the sun was clouded during the rising of the sap, it would visibly subside with great rapidity ; but when the sun-beams again broke out, it would immediately return to its rising state, just like the fluid in a thermometer *. A plant of spearmint, placed in a syphon one-fourth of an inch in diameter, absorbed, during the day, so much water, that it fell into the opposite end to the depth of an inch and a half ; in the night only to one-fourth of an inch ; during a rainy day still less ; and, when the thermometer stood at the freezing point, the plant absorbed no moisture at all †. That it is to the influence of heat alone, and not to any other peculiarity in the season, or in the plant itself, that, the other requisites being duly supplied, its vegetation is to be ascribed, we learn from the fact, that trees in a hot-house continue to grow during the winter, while others of the same species in the open air cease to do so. Even the branch of a tree, planted in the open air, will, during winter, when led into the hot-house, undergo the usual vegetative changes, provided its stem be guarded from severe frost ; but, on the contrary, a branch brought into the open air from a tree that is flourishing in the hot-house, will at the same season cease to grow ‡ ;—affording sufficient evidence of the necessity and the influence of heat.

* Statical Essays, vol. i. p. 122. † Ibid. p. 27.

‡ Philosophical Transactions, vol. lxiii. part 1.

25. With regard to light, we may observe, that whatever doubts may exist concerning its effects on the germination of seeds, there can be no question but that it exerts a peculiar agency on the appearance and qualities of plants. M. du Fay is said to have been the first philosopher who remarked how much the green colour of plants depended on the agency of light ; and from an observation made about the year 1760, the late Professor Robison of Edinburgh was led to infer, that it was very actively employed in giving rise, not only to the colour, but to the odour and combustibility of vegetables. Having gone down into a coal-pit in the neighbourhood of Glasgow, he brought up some whitish-looking plants, but no one knew what they were. After being exposed to the light, however, the white leaves died away, and were succeeded by green buds ; and the plant acquired the smell of tansy. On further inquiry, he found, that the sods on which the plant grew had been taken down into the pit from the part of an adjoining garden ; and although the plant had continued to grow in its new situation, yet neither in colour, in odour, nor combustibility, did it at all resemble plants of the same species which had vegetated under an exposure to light. The etiolation or blanching of the roots of celery, and of the inner parts of cabbages and lettuce, are familiar examples of the same kind ; and Mr Davy ascertained, that after the green colour is formed, it will again disappear, if the plant be excluded from light. The leaves of the common lettuce were in six days rendered very pale, by being deprived of light, and at the end of another week they were quite white : the

growth of the plant was checked ; and its leaves, on being analyzed, yielded more carbonic acid and water, and less hydrogen and residual carbon, than an equal weight of green leaves,—results similar to those obtained by Chaptal, Hassenfratz, and Sennebier. The various colours of flowers are ascribed also to the combination of light in different proportions. Red rose trees, if carefully excluded from light before their flowers begin to appear, will yield flowers almost white: and marine plants are governed by the same laws with regard to colour as land vegetables *. Scheele observed, that it is the violet ray which acts most powerfully in reducing the oxide of silver † : and Sennebier ascertained, that the same ray has the greatest effect in producing the green colour of plants ‡ ; but, according to Mr Gough, the presence of oxygen gas is required to aid its operation §. The late Dr Hope found, that the light produced by art effected changes in degree similar to that which issues from the sun, which has since been confirmed by others: and the light of the moon has a similar effect. It follows, therefore, from these facts, that although light is not immediately necessary to the growth of plants, yet that many of their more peculiar and characteristic qualities depend essentially upon its action.

* Davy in Beddoes' Contributions to Science, p. 186. et seq.

† On Fire, p. 98.

‡ Mem. Phys. Chim. vol. iv. p. 72.

§ Manchester Memoirs, vol. iv. p. 501.

26. But although water and heat alone be the first requisites to the commencement of vegetation, yet they are not of themselves sufficient to carry on that process. Air likewise is required. Mr Papin put an entire plant into the exhausted receiver of an air-pump, and it soon perished; but on keeping the whole plant, except the leaves, which were exposed to the air, in this *vacuum*, it continued to live a long time *. Dr Hales, Du Hamel, and Mr Knight, found that plants do not vegetate when deprived of their leaves: and it is well known, that when the leaves of a plant or tree are destroyed by insects, its vegetation for that season is suspended, and the fruit gradually decays. We have already seen, that when the upper surface of the leaf was covered with varnish or laid on water (23.) it died in a few days; and Dr Darwin smeared the upper surfaces of the leaves over with oil, and the plant died in a day or two after †. As thus without air plants do not vegetate, and as they cease also to grow when deprived of their leaves, even although air be supplied, we must conclude, not only that air is essential to the growth of plants, but that their leaves are the organs which act upon this air. And farther, as their death takes place if the upper surfaces of their leaves be smeared over with varnish or oil, we must also conclude, that it is to some action going on between those surfaces and the air that the continuance of their life is to be ascribed. Let us next then inquire into the

* Darwin's Phytologia, p. 51.

† Ibid. p. 45.

nature of this action, or the changes which the air undergoes in the process of vegetation.

27. Dr Priestley, whose experiments and discoveries first opened the way to an investigation of the reciprocal changes which take place between living vegetables and the air, was led to conclude, that plants not only lived in nitrogen gas, or what he called phlogisticated air, but that, during their growth, they restored to the air the pure part of which it had been previously deprived by the processes of combustion and respiration, and by the decomposition of organized bodies. He found, that if a sprig of mint was put into a jar of air inverted over water, and in which a candle had previously burned out, the air was in a few days so far restored that another candle would burn in it perfectly well. He also put a sprig of mint to grow in air wherein mice had died, and in eight or nine days a mouse would again live very well in this air, though he died in another portion of the same air in which no mint had grown. In air strongly tainted by putrefaction, sprigs of mint have sometimes presently died; but if this do not soon happen, they thrive in a surprising manner. These facts led him to conclude, that plants, by their vegetation, reverse the effects produced on the air by the several processes above described, and thus become the chief means by which the purity of the atmosphere is maintained*.

28. The experiments from which this conclusion was drawn, were made so early as the year 1771,

* Observations on Air abridged, vol. iii. p. 251. et seq.

before Dr Priestley had discovered any correct means of analyzing the air ; nor does he seem to have been aware, that the effects produced on the air by the living functions of animals are very different from those which arise from the decomposition of organized bodies. However much, therefore, we may be disposed to admire the beautiful provision which this supposed reciprocity of action would establish between the functions of animal and vegetable life, we must suspend our belief in its truth until better proofs are brought forward than these experiments afford. Even the fluctuation of opinion on this point which Dr Priestley himself experienced, may well suggest doubts of the correctness of his views ; for towards the end of the same year, some experiments of the same kind, he observes, did not answer so well, and the restored air relapsed to its former noxious state, which led him to suspend his judgment concerning this power in plants *. In 1772, on resuming his experiments, he had the most indisputable proof of the restoration of putrid air by vegetation ; but hearing that Mr Scheele and others had obtained different results, he again repeated them in 1778. The experiments of this year were unfavourable to his former hypothesis ; for whether the air had been injured by respiration, the burning of candles, or other means, it was not rendered better by vegetation, but worse ; and the longer the plants continued in the air, the worse it became. He tried a great variety of plants, as sprigs of mint, spinach,

* Observations on Air abridged, vol. iii. p. 251. to 263.

lettuce, and some others ; but with no better success. His method was to put the roots of the plant into a phial filled with earth and water, and then to introduce it through water into the jar containing the air on which he was making the experiment. In some cases, the air was certainly meliorated, but Mr Scheele, he adds, always found it injured : and he concludes, that, upon the whole, it is *probable* that the vegetation of healthy plants, growing in natural situations, has a salutary effect on the air in which they grow *. Thus we see, from Dr Priestley's own representation of facts, related with his usual candour, that nothing certain can be inferred from his experiments in favour of this supposed purifying power in plants : and that his conclusion was, even at that period, in direct opposition to the experience of the celebrated Scheele.

29. Notwithstanding, however, the uncertain, and, in many respects, contradictory evidence on which this conclusion has been shewn to rest, few opinions in modern science have obtained a more general belief : and both physiologists and chemists seem, in this instance, to have satisfied themselves with contemplating at a distance the beauty of the *final cause*, instead of approaching to a nearer examination of the facts on which the opinion has been maintained. Accordingly, this opinion still keeps its ground, and, in no publication that we have seen, has its accuracy been even questioned. In the experiments, however, which we are about to detail, it will be

* Observations on Air abridged, vol. iii. p. 273. et seq.

seen that their results are altogether opposed to the notion that plants, by their vegetation, purify the air ; and these experiments have been so often repeated, and with such uniformity of result, as to impress us with an entire confidence in their truth. We proceed, therefore, to investigate the actual changes which atmospheric air undergoes in vegetation ; and, first of all, the effects produced on its nitrogenous portion. It has been already shewn, that seeds (5.) do not in the smallest degree germinate in nitrogen gas : and Dr Ingenhousz proved, also, by experiment, that, in pure nitrogen gas, plants do not grow. This conclusion has been more lately confirmed by the experiments of Mr Gough. He confined several succulent plants in jars of nitrogen gas when their flowers were just ready to expand ; but they died away without putting forth any of their blossoms. Others were placed in similar situations before the flower-buds formed upon them, but made not the least effort towards vegetation ; and many more, which grew very well in inverted jars of atmospheric air, ceased to vegetate when transferred into jars of nitrogen gas. But this gas, although it does not aid vegetation, appears in no degree to injure the faculty of growth in plants any more than in seeds (5.) ; for a slip of spearmint, which had remained twelve days in nitrogen gas, recovered upon being restored to the air ; and an offset of *sempervivum* vegetated freely after being removed from a jar of the same gas, in which it had been kept from the 2d of April to the 2d of May *. As

* Nicholson's Journal, November 1804, p. 218.

thus it appears, that nitrogen gas exerts no direct influence on plants, so it is probable that they produce no change upon it; and we may conclude, therefore, that this gas, in its simple and uncombined form, is altogether inactive in the process of vegetation.

30. We have next to inquire into the necessity and use of oxygen gas, and the changes which it undergoes in vegetation. It has been shewn by Dr Ingenhousz, that plants do not, any more than seeds, grow in any species of air unless it contain a portion of oxygen gas, and this gas have access to their leaves: that they vegetate very well in atmospheric air and in oxygen gas; and, therefore, that this latter gas is necessary to vegetation*. What then are the changes which it suffers in that process? By the experiments of Dr Woodhouse, we are taught, that plants growing in earth and confined in a glass vessel of atmospheric air render it impure, and that carbonic acid is formed: that when confined in oxygen gas, previously well washed in lime-water, carbonic acid is, in like manner, produced: and that the quicker this acid is generated, the sooner does the plant die†. These facts derive confirmation from the following experiment. Some peas, growing in a small pot of mould to the height of several inches, were placed under a jar of atmospheric air inverted over water; and a common wine-glass, the foot of which was broken off, was filled with lime-water,

* Thomson's System of Chemistry, vol. iv. p. 290.

† Nicholson's Journal, July 1802, p. 152.

and inserted into the mould in the midst of the growing peas. The lime-water soon acquired a pellicle on its surface, which, by the fourth day, was very thick, and, by the sixth day, had sunk down to the bottom of the glass; and the water into which the jar was inverted, had risen in it to a considerable height,—proving that carbonic acid was formed, and that a diminution in the bulk of air had taken place.

31. As the mould, in which these plants grew, might by some be supposed to influence the production of the carbonic acid, the experiment was next varied in the manner following. Some mustard seeds, which were raised on moistened flannel, and had grown to more than an inch in height, were introduced into a jar of atmospheric air, and supported about half way up it by a small whalebone hoop, covered with netting. In a deep dish, filled with water, and appointed to receive the inverted jar, was placed a glass of lime-water, which stood directly under the hoop. In four hours, a thickish pellicle overspread the lime-water, and the inside of the jar was bedewed with moisture. At the end of twenty-four hours, the lime-water was very turbid, and the water, into which the jar was inverted, had risen nearly half an inch. These appearances increased during the two succeeding days, at which time the water stood nearly an inch high in the jar. The plants now began to look sickly, gradually drooped, and, by the ninth day, fell down in different directions against the sides of the jar, and, when withdrawn from it, emitted a putrid smell. The residual air extinguished a taper, rendered lime-water

turbid, and suffered thereby a farther loss of bulk. This experiment was repeated, by confining mustard plants in the same manner in jars of air inverted over mercury. The lime-water was, in like manner, rendered turbid, and the mercury for two or three days continued to rise into the jars : after a certain time, however, it became stationary, and then began to fall, and the plants when withdrawn yielded a putrid smell.

32. As by the preceding experiments it is proved, that the pure part of the air disappears in vegetation, and that carbonic acid is produced, it is desirable to know the extent to which this destruction of the oxygen gas proceeds. Some mustard seeds, therefore, which had grown on moistened flannel to the height of an inch, were conveyed into a jar, and supported about half way up it by a small hoop in the manner already described. The jar held 36.5 cubic inches of air, and was inverted into a saucer, in which was placed a small glass-cup, containing one cubic inch of the water of potassa : mercury was then poured into the saucer, so as to surround the mouth of the jar. Three slips of paper, graduated to inches and tenths, were pasted at equal distances on the outside of the jar, to mark the ascent of the mercury : and the whole was set aside in a room where the barometer was at 29.2 inches, and the thermometer at 61.5°. The inside of the jar was presently bedewed with moisture, and, by the close of the twenty-fourth hour, the mercury had risen in it $\frac{6}{10}$ of an inch, bearing up the alkaline solution with it. By the forty-eighth hour, the mercury stood at one inch, when that in the saucer was brought to a level with

it ; and at the end of the third day, it had undergone no variation. At this period, the barometer was 29.75, and the thermometer 68° .

33. In order to examine the air, the saucer and jar were now immersed in the pneumatic trough, and water being admitted into the jar, the mercury fell, and the cup with its solution was withdrawn through the water : the solution effervesced briskly on the addition of diluted sulphuric acid. A portion of the residual air being then transferred into a small jar, was found to extinguish a lighted taper that was plunged in it : and another portion was shaken with lime-water without producing or suffering any change ; neither did it afterwards experience any perceptible loss by agitation with the liquid sulphuret of potassa, nor by being placed in contact with a stick of phosphorus. The result, therefore, of this analysis teaches us, that, during the growth of the plants, the oxygenous portion of the air gradually and completely disappeared ; and that the carbonic acid that was formed was entirely attracted by the water of potassa employed.

34. In the process of germination it has been shewn, that the bulk of carbonic acid produced, bears a constant proportion (7, 8.) to that of the oxygen gas which disappears : and to ascertain whether the same did not hold in vegetation, a similar method was adopted. After the foregoing analysis of the air was completed, the jar was allowed to run dry, and being then re-inverted, it was set on its bottom : and the plants, together with the glass-cup, and a bulk of water equal to the solution, were re-placed in it. Water, to the quantity of 25.4 cubic inches,

was then poured in till it reached the height to which the mercury had risen, which, therefore, was the bulk of residual air : and afterwards to fill the jar completely, it required 7.6 cubic inches more, which was the bulk of air that had disappeared. These quantities together amount only to 33.0, and the 3.5 required to make up the 36.5 cubic inches of air which the jar was capable of containing, must, therefore, be allowed as the space occupied by the several substances used in the experiment, which reduces the volume of air actually employed to 33.0 cubic inches, of which a portion equal to 7.6 had disappeared. The barometer, at the end of the experiment, was, as already stated, 29.75, and the thermometer 68°. Hence, therefore, to make the necessary reductions, we have $\frac{25.4 \times 29.75}{29.2} = 25.8784$, but $\frac{6.5 \times 25.8784}{483} = .34826$ and $25.8784 - .34826 = 25.53014$, the corrected volume of air at the close of the experiment. But atmospheric air contains $\frac{22}{100} = \frac{1}{4.54}$ of oxygen gas, and $33 - 25.53014 = 7.46986$ and $\frac{7.46986}{33} = \frac{1}{4.417}$. Or if we compare the diminution which the air suffered, with the quantity of oxygen gas which it originally contained, we have 100 : 22 :: 33 : 7.26, which comes near to 7.46, the estimated amount of the diminution that actually took place. In a second experiment, conducted in a similar manner, the loss of bulk which the air suffered, after the necessary reductions were made, was $\frac{1}{4.1}$. From these facts it is inferred, that the loss of bulk, which a given volume of air suffers in vegetation, corresponds very nearly with the proportion of oxygen gas which that air contained : and if this

loss be attributed entirely to the abstraction of carbonic acid, the quantity of acid produced will rather exceed that of oxygen gas lost. But on passing into carbonic acid, this gas suffers (11.) a small degree of condensation; and whatever future experience may determine this to be, it must of course be deducted from the whole diminution which the air suffers, and consequently will diminish somewhat the quantity of carbonic acid formed.

(35.) Having thus endeavoured to trace the nature and extent of the changes which the air suffers in vegetation, let us next direct our attention to the manner in which they take place. Through the whole of the preceding experiments (30. et seq.) we have seen that oxygen gas is essential to vegetation, and that it gradually and completely disappears, when, in a given bulk of air, that process is made to take place. What then becomes of it? By many it is supposed to be *absorbed* by a necessary function of the plant. It may be worth while, therefore, to inquire what is that structure of the plant by which such an absorption can be effected? Grew and Malpighi speak of *spiral vessels* which they discovered in the wood, and which later inquirers have found to extend to the minutest branches, and to spread through every leaf. From these vessels being always found empty, they supposed them to be air-vessels, and called them *tracheæ*; but Dr Darwin observes, that if the end of a vine-stalk, two or three years old, be cut horizontally, these vessels may, by the help of a common lens, be seen to be full of juice, which, in a minute

or less, passes on or exhales, and then the vessels appear empty, that is, filled with air. The same author, however, speaks of *horizontal vessels*, first noticed by Malpighi, which pass through the bark of trees to the alburnum, and, as he supposes, admit oxygen gas *; but, besides that these vessels are not proved to extend to the leaves, which alone operate changes on the air, we have seen that the leaves of themselves are able to preserve the life of the plant when the stem is confined *in vacuo* (26.); and, on the contrary, that the stem is not sufficient, when deprived of its leaves, or these are smeared over with oil. Lastly, Dr Hales readily caused air and water to pass through a piece of recent stick, by setting one end of it in a cup of water placed under a receiver, and then exhausting the receiver of its air, and these vessels he imagined to perform a respiratory function; but the same vessels were found by Malpighi to be present in the roots, where they are not exposed to the air, and cannot, therefore, serve the purpose of respiration. Neither, as before remarked, have we any proof that any other part than the leaves of the living plant act upon the air in contact with it to an extent necessary to maintain its life.

36. Have then these leaves any organization by which an absorption of air can be effected? It is ascertained that the sap of plants ascends to the leaves, and after undergoing certain changes in them, is again returned to the branches and stem. Dr Dar-

* Phytologia, p. 12, 13.

win placed a plant of spurge (*Euphorbia Helioscopia*) in a decoction of madder, and in a few days the coloured fluid passed along the middle rib of the leaf, and ran by several branches to the extremity of it upon the upper surface; while, on the under side, a system of vessels was seen coming from the end of the leaf, and conveying a milky fluid, which descended finally into the foot stalk*: and Mr Knight has traced the vessels conveying this peculiar juice from the leaves into the cortical layers of the inner bark†. Now the difference in the appearance and qualities of this returning fluid was probably effected in the leaves by the agency of the air, and in this way the *succus proprius* of plants seems to be prepared. But can we suppose, either the air or its oxygenous portion to be received into these minute vessels, already filled with fluids, to be there decomposed, then transformed into, and at length emitted as carbonic acid? If so, by what passages can it enter, and how afterwards can it be expelled? It cannot enter by the transpiring or exhaling pores, because the fluids constantly escaping from them must obstruct its admission; nor have we any instance of any organ performing two contrary actions at one and the same time,—a condition which this supposition would involve. If, again, it enter by peculiar vessels fitted for the absorption of air, (whose existence however is not proved), by what vessels can it afterwards be emitted as carbonic acid? Not surely

* Phytologia, p. 43.

† Philosophical Transactions, 1801, p. 337.

by the same vessels which absorbed it, for the reason above given : and, therefore, the existence of other peculiar vessels for the emission of air, ought likewise to be proved. By the common absorbents of the leaf, it cannot be supposed to enter ; for they are employed in taking up fluids, and are besides situated on the inferior surface (23.), while the changes on the air are effected by the superior only. To suppose that it gains admission through inorganic pores, is contrary to all sound physiology ; for we have no example of any healthy function being carried on by animated beings, otherwise than by a living action, and by a peculiar organization destined to perform that action.

37. Rejecting, therefore, the opinion that any aëriform fluid obtains admission into the vascular system of plants by any process resembling the living function of *absorption*, let us next inquire how far the supposition of *chemical affinity* will help to remove the difficulty. The juice of the leaves, which is supposed so powerfully to attract the oxygen gas of the air, is contained within its peculiar vessels, and these vessels are likewise covered by the green colouring matter, over all which is spread the *epidermis*. The minute structure of these parts has been lately described by M. Jurine, as it appeared on microscopic examination * ; but the appearances observed by him are so different from those represented by other authors, as to justify, in his opinion, the adoption of new names, in almost every instance, to

* Phil. Mag. vol. xvi.

express them,—a circumstance which, while it begets a doubt of their accuracy, tends greatly to puzzle the student, without proportionally advancing the science. It is sufficient for our purpose to observe, that no direct communication is demonstrated to exist between any pores on the surface of the epidermis, and the vessels in which the vegetable fluids move. If, therefore, chemical affinity operate here, it must be between substances, not, as usual, in actual contact, but at a certain assignable distance; and that distance intercepted by a very compounded organized structure. Let us, however, suppose for a moment the oxygen gas of the air to obtain admission into the juices of the plant, under all these circumstances, by the operation of chemical affinity;—by what power will the carbonic acid, which it is there supposed to form, be afterwards emitted? Not by chemical affinity, surely, for where is the external agent to effect it? Not through a living action, for where is the structure to perform it?—Or how can the reception of air be thus carried on by one law, and its expulsion by another so widely different? How, also, is this oxygen gas, previous to its supposed attraction into the plant, to be separated from the nitrogen gas with which it is naturally united? If, as is probable, the separation be brought about only by the superior affinity of carbon furnished by the leaf, then the carbonic acid, instead of being emitted from the leaf, is formed exterior to it, and out of the very oxygen gas which is supposed first to enter into the leaf to form it. Those, also, who favour this opinion of the attraction of oxygen gas, cannot well suppose that plants likewise

attract carbonic acid, retaining the carbon, and giving out the oxygen gas; for this would not only be making an attraction and expulsion of oxygen gas to go on at the same time, but would be doing the same by the carbonic acid also: and how could that acid be ever formed and emitted at all, if the oxygen gas which enters into it is first singly expelled, and the carbon, which is its other constituent, is permanently retained? And if we suppose, that, during the day, carbonic acid is received and oxygen gas expelled by the leaves, and that during the night the exact contrary takes place*; it seems very difficult to account for the death of plants (30.) confined in a given bulk of air at all, since they would, in fact, be weaving Penelope's web, undoing by night what they had done by day, and *vice versâ*. All these difficulties are greatly increased by the supposition, that the air enters into the vessels of plants in its *undecomposed* state; for since the nitrogen gas remains unaffected (29.), what useful purpose could its reception answer? Or by what means, when separated from its oxygen gas, could it again be expelled from the plant in the exact bulk which it before occupied? On these grounds, we deny the entrance of aëriform fluids into the vascular system of plants, by any power analogous to chemical affinity.

38. If from the experiments already related (32.), it be granted, that, during vegetation, the oxygen gas of the air gradually and completely disappears, and that a bulk of carbonic acid, nearly

* Thomson's System of Chemistry, vol. iv. p. 291.

equal thereto (33.) is formed ; and if it be admitted, that none of this gas is received into the vessels of the plant (35, 6, 7.), we must embrace the belief, not only that the acid is formed out of the oxygen gas originally present in the air, but that it is not emitted ready formed by the plant. If the acid were formed independent of this oxygen gas, the whole bulk of air ought to be increased, instead of being diminished, by vegetation : if it be not so formed, then as no oxygen gas is received into the plant, no carbonic acid can be emitted from it. Plants also, in nitrogen gas, do not vegetate, and cannot, therefore, by a living process, form carbonic acid ; and when, in atmospheric air or in oxygen gas, this acid is formed by vegetation, that gas disappears, and the plant for a time lives : whence it follows, that without the presence of oxygen gas, the living plant is unable to form carbonic acid, and that this acid, therefore, is formed by the reciprocal changes going on between this oxygen gas and the plant. If we do not, indeed, admit the conversion of oxygen gas into carbonic acid, there is no way in which its loss can be accounted for, since there is no other new product formed, and none of the gas, as we maintain, enters into the vascular system, and combines with the fluids of the plant. In all the foregoing experiments also, the deterioration of the air increased in proportion to the continuance of the vegetative process, until the whole of its oxygen gas completely disappeared ; and the gradual rising of the mercury into the jar (32.), indicated a corresponding production of carbonic acid, until the bulk of the latter nearly equalled that of the former ;

which reciprocal action can in no way be accounted for, but by that conversion of the one gas into the other, which is here supposed actually to have place.

39. To effect this conversion, however, of the oxygen gas, carbon must be from some source supplied. This Dr Woodhouse supposes, as in the case of seeds, to proceed either from the organized remains of the soil in which the plants grow, or from the carbon of the decayed leaves, when confined a considerable time in a given bulk of air* : but it has been shewn, that carbonic acid is equally produced (31.), where there is no soil present to afford carbon; and that it appears also in a few hours (31.), and before the leaves, therefore, exhibit any sign of decay. Indeed, there does not appear any good reason why oxygen gas should be so essential to the life of vegetables, if it could be acted on or changed by those parts of them only which are already dead. To the living plant, therefore, as well as to the seed (20.), we must look as affording carbon, and by the union of this carbon with the oxygen gas of the air, must we consider the carbonic acid met with in vegetation to be formed.

40. But as this carbonic acid is produced in vegetation, the oxygen gas proportionally disappears, (30. et seq.), and when this substitution is complete, the plant gradually declines and dies. To what cause is this effect to be immediately ascribed? It must arise from the superabundance either of nitrogen gas, or of carbonic acid, or from the defi-

* Nicholson's Journal, July 1802, p. 152.

ficiency of oxygen gas. Now, it has been shewn (29.), that although plants do not grow in nitrogen gas, yet that gas suspends only, but does not destroy the vegetating faculty : and precisely the same occurs when they are placed *in vacuo*, where no direct effect can be attributed to any air at all. In all the preceding experiments also, vegetation did not cease till all the oxygen gas had disappeared ; and, therefore, the superabundance of nitrogen gas did not prove fatal so long as any oxygen gas remained. Mr Gough, indeed, has shewn (29.), that certain plants retain the faculty of growth after being confined for weeks in nitrogen gas. To the want or absence, therefore, of oxygen gas, and not to the presence of nitrogen gas in any proportion, is the death of plants in a given bulk of air, as well as that of seeds (21.), to be entirely attributed. How far the proportions of nitrogen and oxygen gas, as they exist in atmospheric air, may be the best adapted to the actual structure and constitutional habits of plants, we have not experimentally attempted to determine : but the general analogy to be derived from the case of seeds (14, 15.), and the fitness of means to the perfection of an end observed throughout all the operations of nature, lead irresistibly to the belief, that these proportions are in reality the best.

41. The only other gas present in the preceding experiments, by which the growth of plants can be affected, is carbonic acid : and the experiments of Priestley *, Ingenhousz, and others, clearly evince,

* On Air, vol. i. p. 36. and iii. p. 310.

that when confined in this acid, plants will die. This, however, is no more than what we have seen to take place in nitrogen gas (29.), and *in vacuo*, and cannot therefore be received as proof of the positive operation of carbonic acid. From the experiments of the late Dr Percival, and Mr Henry, we learn also, that although plants, when wholly confined in carbonic acid, certainly died, yet that where a small portion of oxygen gas was admitted, they as certainly lived and flourished *. Neither do plants, confined in a given bulk of atmospheric air, die till all the oxygen gas disappears (30.); nor, consequently, does the quantity of carbonic acid formed prove fatal so long as any oxygen gas remains. So far, indeed, is carbonic acid from being fatal to vegetation, that many have deemed it highly *salutary* †, and some even contend that it is *essential* to that process,—alleging that it is absorbed by the leaves of plants during the day, decomposed within their vessels, its oxygen being afterwards emitted, while its carbon is retained as food ‡.

42. Against this opinion of the absorption and emission of gases by the leaves of plants, when growing naturally in air, we have already, both on physiological and on chemical grounds, been induced to enter our protest: but the importance of the question, together with the high authorities by which it has been supported, will, we trust, plead in excuse

* Manchester Memoirs, vol. ii. p. 341. et seq.

† Ibid. vol. ii. p. 331.

‡ Thomson's Chemistry, vol. iv. p. 283. et seq.

for the additional remarks which the present occasion enables us to offer. Passing over, then, for the present, the anatomical and chemical difficulties which beset this opinion, let us, for a moment, admit the capability of the leaves to absorb or attract carbonic acid; and then examine how far such a supposition is consistent with reason and with fact. That the same substance, carbonic acid, should, during the day, be absorbed by the leaf, and decomposed within it as salutary; and, during the night, should be formed within the same leaf, and emitted from it as noxious*, seems to be not only inconsistent, but absurd. Where would be the advantage in the carbon of the acid being retained for twelve hours as food, if, for the next twelve, it must again be given out as excrementitious? Or where is there an instance, in the whole circle of existence, of a living agent not only first forming its own food, but feeding on its own excretions? If this carbon were, during the day, retained as food, whence comes *that* composing the acid which plants, when confined in a given bulk of air (31.), are constantly forming? If oxygen gas, as these chemists suppose, be during the day constantly emitted, why does that gas gradually disappear as the process of vegetation proceeds (32, 3.)? And why at last is none to be met with, although there is present an abundance of carbonic acid, out of which it is supposed to be formed? It has been proved, that during the day carbonic acid, by the act of vegetation (32, 3.), is constantly forming;

* Thomson's Chemistry, vol. iv. p. 283.

but if, at the same time, it be as constantly absorbed by the leaves, how can its presence be manifested in such quantity, and in such progression, as experiment evinces that it is? If plants do die in a given bulk of air, and that, too, more or less rapidly as carbonic acid is produced (30.), with what propriety can we hold that substance to be essential to their life? If they do live and flourish in air deprived of carbonic acid (30.), and this acid be a consequence of their growth, on what principle can we at the same time assign it as a cause; or how can we consider that substance to be essential to the existence of this process, when it did not itself exist until produced by the very process to which its existence was essential.

43. But it is said, that M. Saussure, by putting a quantity of lime into the glass-vessel in which plants were vegetating, found that they no longer continued to grow, and that the leaves in a few days fell off*. Admitting the correctness of this experiment, is it any proof that the process ceased from the want of carbonic acid? Would not the lime equally abstract the moisture of the plant, and would not this be more likely to occasion the fall of the leaf? The leaf, indeed, might die from the absence of any elastic fluid essential to its life; but, if it did, it would still adhere to the stem. The following experiment is offered in support of this opinion. Two equally sized pots of earth, containing four growing peas each, were placed under two equal jars of atmosphere.

* Thomson's Chemistry, vol. iv. p. 283.

ric air, one of which was inverted over mercury, and the other over water. Each of the pots was surrounded by a quantity of powdered lime, spread on the board by which it was supported. The leaves of the plants in the mercurial jar continued green for several days, and became at length very much curled, but the plants themselves had not increased in height; and when withdrawn from the jar on the sixth day, their leaves were dry to the feel, crumbled between the fingers, and were still very green. The jar, during the greater part of the time, remained dry and transparent; the lime was much increased in bulk; and the mercury had risen considerably into the jar. In the other jar, the water, by the second day, had risen so as to moisten the lime, and its whole inside was quite bedewed with moisture, which appearance it continued to exhibit through the whole period of the experiment. The leaves of the plants did not continue dry or green as in the former case, but assumed a yellowish appearance, and, by the fifth day, the plant had increased an inch and a half in length, the leaves becoming still paler and more yellow; and when withdrawn from the jar, on the sixth day, they were quite moist to the feel. From these experiments, it appears, that lime, placed in jars with growing plants, abstracts their moisture, and thereby checks their growth; but that if the lime be previously moistened, the growth of the plants is not checked, and their moisture is as great as usual. But water is as necessary to vegetation (23.) as air; and, therefore, granting unto lime the power of attracting carbonic acid as well as water, the death of the plants in M.

Saussure's experiments, may with as much justice be attributed to this abstraction of water, as to that of the carbonic acid.

44. That this event did not arise from the abstraction of the carbonic acid, is farther proved by the experiments which follow. Two glass-jars, each containing about 40 cubic inches of atmospheric air, were inverted over water, and into each of them was introduced some mustard plants, growing on flannel, which were supported, as before, by a small hoop fixed half way up the jar. Under the hoop in one jar was placed a small cup, containing about an ounce of water of potassa : the other jar contained the growing plants only. In 24 hours, the water in the jar with the alkaline solution had risen 7-10ths of an inch, and in 48 hours $1\frac{1}{2}$ inch ; but in the other jar, in the same time, it had risen only 6-10ths of an inch. The plants and alkaline solution were now withdrawn under water from the first jar, and five cubic inches of the residual air being then passed into the eudiometer filled with lime-water, produced not the smallest discoloration, nor suffered the least diminution in bulk ; but the same quantity of the air of the other jar, being afterwards treated in like manner, rapidly made the lime-water milky, and suffered a loss of nearly 1-5th of its bulk. The air of neither jar, after washing with lime-water, experienced any loss of bulk by agitation with the sulphuretted solution of potassa, and in all respects the plants in both jars presented the same appearance, a few only reviving on exposure to fresh air. In both these jars, therefore, carbonic acid was formed by the act of vegetation, but in one case it

was abstracted as soon as formed by the alkaline solution, while in the other it remained in contact with the plants; but neither the presence nor the absence of this acid caused any difference in the growth or appearance of the plants; and their decline is referable only to that complete abstraction of oxygen gas, which has been before shewn to be fatal to the vegetative process: hence then carbonic acid, applied to the leaves of plants, is not essential to vegetation, neither is it destructive to the continuance of that process.

45. All that has been hitherto said, applies to the circumstances of plants growing naturally in air: when they are placed in water, other phenomena arise, from which have been drawn arguments in favour of an absorption and emission of gases by leaves. Dr Priestley first advanced the opinion, that plants, in certain circumstances, emitted oxygen gas; and Ingenhousz soon after discovered, that the leaves of plants, when immersed in water, and exposed to the light of day, produced an air which he announced as oxygen gas. But whatever may be the results of these experiments on plants immersed in water, they are not necessarily to be received as proof of the same actions being performed by the leaves when growing naturally in the air: on the contrary, it has been shewn by direct experiment (33.), that when plants are confined in a given bulk of atmospheric air, they gradually and completely destroy its oxygenous portion, which could not possibly happen if they possessed the power of emitting oxygen gas. Moreover, the experiments of Dr Ingenhousz himself teach us, that this supposed emission of oxygen

gas does not depend so much on the power of the leaves, as on the quality of the water in which they are immersed ; for if the water be previously boiled, little or no oxygen gas is collected. River water affords very little gas, but pump water is the most productive of all : and Sennebier also proved, that if the air be previously deprived of all its air by boiling, the leaves of plants immersed in it do not emit a particle of air *. But not only must the water previously contain air, but it must contain carbonic acid ; for Sennebier has shewn, that no oxygen gas is yielded by leaves when plunged in water destitute of carbonic acid : that the quantity of oxygen afforded is proportional to the quantity of carbonic acid which the water contains ; and that when the water loses the power of affording oxygen gas, all the carbonic acid which it contained has disappeared. These experiments prove, that the oxygen gas, which is separated when the leaves of plants are immersed in water, depends altogether upon the presence of carbonic acid †.

46. But surely it cannot be maintained by any one, who for a moment considers the structure and living functions of vegetables, that the leaves of plants, when, as in these experiments, they are separated from the branches, and wholly immersed in water, absorb and decompose this carbonic acid as a natural and healthy function, and afterwards emit its oxygen and retain its carbon. Not only, in these circumstances, must the circulation of their fluids be

* Thomson's Chemistry, vol. iv. p. 284. † Ibid.

completely suspended, but the plants must be entirely cut off from the contact of atmospheric air, which has been shewn to be essential to their life; and if the separated leaf speedily dies when its upper surface only (23.) is laid upon water, what reason have we to conclude that its life can be preserved by immersing it completely in that fluid. Even granting that the production of oxygen gas, in these experiments, was effected by a natural function of the leaves, no sensible advantage, depending on this power, can be supposed to arise to those plants which flourish in the open air; for the carbonic acid, from the decomposition of which alone the oxygen gas is derived, exists in the atmosphere in a quantity infinitely too small to be productive of any good effect: and we have shewn, that instead of absorbing carbonic acid from the atmosphere, plants, by their vegetation, are constantly producing it; and instead of emitting oxygen gas, they are at all times converting it into this very acid. Neither, after all, is it clear that the oxygen gas, which is thus obtained by the decomposition of carbonic acid, depends on any peculiar operation of the leaves; “for light is an essential agent in the decomposition, and it is probably by its agency, or by its entering into combination with the oxygen, that this substance is enabled to assume the gaseous form, and to separate from the carbon *.”

47. That the organized structure of the leaves is not at all necessary to effect the separation of air from

* Thomson's Chemistry, vol. iv. p. 285.

the water in which they are immersed, may be inferred also from the experiments of Count Rumford, who found that dried leaves, fibres of raw silk, and even of glass, when placed in similar circumstances, produced a like separation of air. This air varied in quality, being in many cases less pure than that of the atmosphere * ; and Dr Woodhouse found river water to yield chiefly nitrogen gas †. This variation arises no doubt from the nature of the air which the water contains ; for Mr Dalton remarks, that although atmospheric air, expelled from pure water, contains 38 *per cent.* of oxygen, yet that, by stagnation, it loses a part or all of it, notwithstanding its constant exposition to the atmosphere : and this must arise from some impurities in the water which combine with the oxygen, since pure rain water lost none of its oxygen after standing in a bottle more than a year ‡. Hence, then, we see, that to effect the separation of air from water, the organized structure of the leaf is not only not necessary, but that the quality of the separated air is altogether different from what this supposed function of the leaves ought to supply. No proof, therefore, of the absorption and emission of gases, much less of oxygen gas, by the natural functions of leaves, can be derived from these experiments on plants immersed in water : and were the experiments even more precise, they would not in the least apply to the case of vegetables which flourish in the open air.

* Philosophical Transactions, 1787.

† Nicholson's Journal, July 1802.

‡ Manchester Memoirs, vol. i. new series.

CHAP. III.

OF THE CHANGES INDUCED ON THE AIR BY THE RESPIRATION OF INSECTS, WORMS, FISHES, AND AMPHIBIOUS ANI- MALS.

48. **T**o the commencement of living action in vegetables, the presence of water (1. 23.) has been shewn to be essentially necessary, and among several inferior animal beings, its operation is equally striking and apparent. The ova of innumerable tribes of animals, some of which afterwards inhabit the air, are deposited in water, and undergo their various stages of evolution only while exposed to the influence of that necessary fluid. Neither is its agency confined to the earliest states of existence, nor to those animals which may be properly called aquatic; for examples abound, where its operation extends through every period of life, and among animals which reside wholly in the air. Snails in their shells have been thrown into a drawer, and lain by for fifteen years; but reco-

vered action on being immersed in a bason of water *. The gordius, or horse-hair eel, while in water, is in incessant motion, but, if the water dry up, its movements cease, it shrivels up, and may be kept in this state for an indefinite length of time; but place it again in water, it begins to move, and, in a few minutes, is as brisk and lively as ever. There is an animalcule that sometimes resides in wheat, which, after lying dormant for nearly thirty years, has recovered its vital functions, merely by moistening the grain with water. The rotifer (*vorticella rotatoria*), which lives in small puddles of water, often on the tops of houses, shrivels up as the water evaporates, till it becomes like a piece of dried parchment, in which state it may be preserved for years without suffering the smallest change: but on moistening it with water, it resumes its pristine form, and soon becomes as lively as ever. Suffer the water again to evaporate, and the animal dries up as before: but restore to it the moisture, and again it is brought to life. In this way it has been alternately deadened and revived eleven times, without any apparent exhaustion of its vital powers: and although subjected, during its torpid state, to a heat of 56° Reaumur, and a cold of 19° , it was equally susceptible of revivification as at first †. These facts sufficiently demonstrate the necessity of water to the commencement of animal action.

* Philosophical Transactions, 1774.

† Anderson's Recreations in Agriculture, No. x. p. 255. et seq.

49. Equally essential to this action is the presence and operation of heat. The ova of myriads of insects are evolved by its immediate influence; and its power is not less necessary to the maintenance, than to the beginning of living action. When abstracted to a certain degree, numerous tribes of animals pass into a torpid state, and again recover action as the heat is restored. Caterpillars, spiders, and ants, were many times in succession rendered torpid, and again restored to action, in the experiments of Dr Michelotti, by the alternate abstraction and communication of heat *. In a temperature, —1° Reaum. Spallanzani found living action in snails to cease; and the same result was obtained by exposing the marmot to a similar abstraction of heat †. The heart moves quicker in hot than in cold animals, says Dr Irvine; and in many animals, during the severe cold of winter, it does not move at all. The heart of snails, which beats manifestly in summer, was found to be perfectly at rest in winter; and the same thing is observed in many of the fly tribe. Of the same description too, are the serpent and viper tribe, frogs, toads, and tortoises: even the bat, which is naturally a hot animal, becomes, during the winter, as cold as the surrounding medium, and its heart is perfectly at rest ‡. If an egg be opened some days after incubation, so that the *punctum saliens* may come into view, according as it is exposed to heat or cold,

* Phil. Mag. June 1804.

† Memoirs on Respiration, p. 154. 334.

‡ Irvine's Chemical Essays, p. 201.

says Dr Mayhow, you will perceive the *corculum* or heart to pulsate, or to languish and cease from motion *. The hearts of frogs, reduced to torpidity, were removed by Spallanzani, and living action could still be excited in them, by the re-application of a proper degree of heat : and the heart of a turtle, on being put into milk-warm water, was repeatedly observed by Dr Gardiner to yield a tremulous motion, six or seven hours after it was removed from the body, and had become much shrivelled and dried. If suffered to become cold, it was insensible to every stimulus ; but when again warmed in water, it repeated its palpitations on being pricked with a needle †. These and many other facts which might be adduced, sufficiently establish the necessary concurrence of heat to the production of action in animals : and prove likewise, that its abstraction, although causing a suspension of the animal functions, does not necessarily destroy the capacity of their renewal.

50. The direct effects of light in producing the various colours of vegetables (25.) have been distinctly proved : and the experiments and observations of Mr Davy, go to shew that this agent exerts a similar operation on the various classes of animal beings. He has observed, that the zoophyta exposed to light, are uniformly brighter coloured than those which have by any means been secluded from it ; and he succeeded in altering the colour of two sea anemones

* Tractat. Quinq. p. 325. An. 1674.

† On the Animal Œconomy, p. 46.

from a dark red, to a pale pink, by secluding them from light. The parts of fish, which are exposed to light, exhibit various colours; but the belly, which is deprived of light, is uniformly found white in all of them. The birds that inhabit the tropical countries, are much brighter coloured than those of the north: and those parts of the feathers, which are exposed to light, are almost always coloured, while the parts secluded from light are generally pale or white. The same observations apply to the hairs of quadrupeds: and, not only are the beasts of the equatorial uniformly brighter coloured than those of the polar countries, but the northern animals are dark coloured in summer, and white, or pale, in winter*. These observations point out the influence of light, in giving rise to the colour of animals; but they do not, any more than in vegetables, shew the immediate necessity of it to the existence of animal action.

51. But when, by the combined agency of water and heat, the animal structure is brought into a condition fitted for exhibiting living action, the presence of atmospheric air is required to enable it to continue this action; for, as far as observation has extended, no living being can long subsist without a due supply of fresh air. "In insects," says Mr Ray, "there are many orifices on each side of their bodies for the admission of air, which, if you stop with oil or honey, the insect presently dies, and revives no more. This, he adds, was an observation

* Contributions to Science by Beddoes, p. 192.

of the ancients, though the reason of it they did not understand, (*oleo illito, insecta omnia exanimantur*, Plin.), which was nothing but the intercluding of the air; for, though you put oil upon them, if you put it not upon or obstruct those orifices therewith, whereby they draw the air, they suffer nothing*." Mayhow observes, that if the oil be applied only on some of these orifices, the neighbouring parts immediately become paralytic, by being deprived of the nitro-aërial particles of the air, while the other parts, in the meanwhile, continue sound†. Mr Derham found, that wasps, bees, hornets, and grasshoppers, seemed dead in two minutes, when placed under the exhausted receiver; but revived in two or three hours on being restored to the air, even though they had remained *in vacuo* twenty-four hours‡. Of the *vermes* class, snails survive several hours in the exhausted receiver: efts, or slow worms, two or three days; and leeches, five or six||.

52. The same necessity of fresh air in the water in which they live, is required by aquatic animals. Zoophytes, according to Mr Davy, require the presence of air in the water in which they grow, and they act upon it like fishes§. The animalcules in pepper water remained *in vacuo* twenty-four hours: and, being afterwards exposed a day or two to the

* Wisdom of God in the Creation, p. 82.

† Tractat. Quartus, cap. iv. p. 39.

‡ Physico-Theology, p. 8. 7th Edition.

|| Hutton's Mathematical Dictionary, article Air Pump.

§ Beddoes's Contributions to Science, p. 138.

open air, Mr Derham found some of them dead and some alive *. Mr Ray remarked, that fishes cannot live in water without air: they will live in a vessel of water with a narrow mouth for months or years; but if the vessel be stopped, so as wholly to exclude the air, or interrupt its communication with the water, they will be suddenly suffocated †. Dr Priestley confined several small fishes in a vessel, containing three pints of rain water, that had been previously well boiled to deprive it of its air, and they lived only between three and four hours ‡. Mr Davy introduced a large thornback into a jar containing three cubic inches of water, which had been deprived of its air by distillation through mercury: he was very quiet for four minutes and a half, but then began to move about, and, in seven minutes, had fallen on his back, but still continued to move his gills. In eleven minutes, he was motionless, and when taken out, after thirteen minutes, he did not recover ||. Amphibious animals, likewise, cannot live without air, but its deprivation is not immediately fatal to them. Frogs and toads bear the pump for two or three hours, and a frog recovered on exposure to the air, after remaining *in vacuo* seemingly dead for eleven hours §. Hence we see, that, to all these animals, whether inhabiting the air

* Physico-Theology, p. 8.

† Wisdom of God in the Creation, p. 81.

‡ Observations on Air, vol. v. p. 139.

|| Researches concerning Nitrous Oxide, p. 367.

§ Physico-Theology, loc. cit.

or the water, a constant renewal of fresh air is required, while the actions of life continue. What then are the changes induced on atmospheric air by these several classes of animal beings, whereby it is rendered so essential to the maintenance of vital action?

53. For the first, and most accurate knowledge we possess concerning the changes which the air suffers by the respiration of insects, we are indebted to the labours of the celebrated M. Vauquelin. The experiments of this excellent chemist were made on the grasshopper (*gryllus viridissimus*), which is described as having twenty-four stigmata, or breathing pores, ranged parallel with, but exterior to, two white lines, extending longitudinally on the middle of the belly. In this insect they are of an oval form, but they vary in shape in different insects: and it is chiefly by their mediation, that the changes on the air are effected. A female grasshopper was placed in eight cubic inches of atmospheric air: it breathed from fifty to fifty-five times in a minute, and lived thirty-six hours. The air had not sensibly diminished in volume, but, when examined by the test of lime-water, carbonic acid was detected, and after this was removed, the remaining air still extinguished a taper. When many grasshoppers were put at the same time into a given bulk of air, and left till they died, the oxygen gas was nearly, but not entirely, consumed: and phosphorus melted in the residual air when heat was applied, but burned very little. A male grasshopper lived eighteen hours in six cubic inches of oxygen gas: its respiration was oppressive, and it breathed from sixty to sixty-five

times in a minute. The volume of air was not sensibly diminished, but it lost $\frac{5}{100}$ of its bulk by being washed in an alkaline solution *. From these facts, we learn, that insects, by their respiration, consume the oxygenous portion of the air : that carbonic acid is, at the same time, produced ; and that, when all the oxygen gas has disappeared, the animal no longer survives.

54. M. Huber found, that bees very speedily die when put into nitrogen gas, but that they survive in a close vessel of atmospheric air, until almost the last atom of its oxygen gas is consumed †. We likewise confined a number of flies in a flask, containing nine cubic inches of air, and then inverted it into a tall glass of mercury. By the third day, the flies were all dead, and the mercury had risen considerably into the neck of the flask. The residual air lost about $\frac{18}{100}$ by agitation with lime-water, and the remainder did not suffer the smallest diminution by being placed in contact for two days with phosphorus. These results, therefore, agree with those obtained by Vauquelin, and prove farther, that, by the respiration of flies, the whole of the oxygen gas of the air disappears, and that a bulk of carbonic acid nearly equal thereto is formed. The small diminution of bulk also which the air suffered, is to be regarded as a necessary consequence attending the conversion of oxygen gas into carbonic acid, and which, as it accounts for the whole loss the air experienced,

* Ann. de Chimie, tom. xii.

† Mem. sur la Germination, &c. par M. M. Huber et Sennebier.

seems to authorise the conclusion, that, while the oxygen gas had, in this case, completely disappeared, the nitrogenous portion of the air continued undiminished, and probably unaltered.

55. M. Vauquelin proceeded next to investigate the changes produced on the air by the respiration of the *vermes* class of animals. He confined a red slug in twelve cubic inches of atmospheric air, and it lived forty-eight hours. He thinks, that in this animal the breathing pores are situated chiefly behind the head. The air was not sensibly diminished in volume, but it extinguished candles, and copiously precipitated lime from water. Phosphorus was melted in this air, but did not suffer any combustion or change of colour. A snail (*helix pomatia*) was next put into twelve cubic inches of atmospheric air, and lived four days. The oxygen gas entirely disappeared; for the residual nitrogen gas contained not an atom of vital air, and, consequently, phosphorus did not burn in it at all; it contained, however, carbonic acid. Slugs and snails, therefore, require fresh air while in an active state, the oxygen gas of which, by the function of their respiratory organs, is made completely to disappear, and a quantity of carbonic acid is produced, while the nitrogenous portion of the air remains unaltered: and when these changes are effected, living action speedily comes to an end. So exactly do these animals separate the oxygenous from the nitrogenous portion of the atmosphere, that M. Vauquelin suggests the employment of them for eudiometrical purposes*.

* Ann. de Chimie, loc. cit.

56. These experiments of M. Vauquelin were repeated by the late Abbé Spallanzani, and nearly with the same results. That industrious philosopher confined a slug (*limax flavus*) in a given quantity of atmospheric air; and the whole of its oxygen gas entirely disappeared, for the residual air was not in the least diminished by the introduction of phosphorus*. Other living slugs also entirely consumed the oxygen gas of the air, and produced carbonic acid, while the nitrogen gas remained unaltered†. In other instances, however, the whole of the oxygen gas did not disappear during the life of the animal: but, whether this happened or not, the nitrogen gas was, in all cases, left undiminished‡. When placed in pure oxygen, a portion of that gas likewise disappeared, and carbonic acid was, in like manner, produced||. Different species of worms were shut up in a given quantity of air, and they all consumed the whole of the oxygen gas it contained, and carbonic acid was always produced: and when pure oxygen gas was employed, more of it disappeared, and carbonic acid was in proportion produced§. The results of all these experiments coincide completely with those of M. Vauquelin, related in the preceding paragraph: they prove that the oxygenous portion of the air entirely disappears, that carbonic acid is produced, and that the nitrogen gas continues unaltered.

57. Different species of snails were next submitted to experiment by the same author. One of

* Memoirs on Respiration, p. 241.

† Ibid. p. 253.

‡ Ibid. p. 258.

|| Ibid. p. 253.

§ Ibid. p. 68—70.

these animals was confined in seven cubic inches of air, inverted over mercury, for the space of six days, in a temperature varying from 7° to 8° Reaum.: and the bulk of air was sensibly diminished. A quantity of the residual air was then introduced into an eudiometrical tube, filled with mercury, so as to occupy one hundred parts: it was afterwards washed in lime-water, and re-introduced into the eudiometer, on which the mercury rose to 11° . Phosphorus was next inflamed in the remaining air; and when every thing was again brought back to the former temperature, the mercury had risen to $11\frac{1}{3}^{\circ}$ *. This elevation of the mercury, he adds, is not very sensible when phosphorus is employed, but when equal quantities of the residual air, and of nitrous gas were mixed together, so as to occupy two hundred parts, the diminution was four, five, or six of these parts, which indicated the quantity of oxygen gas the air contained †. From these results, he is led to conclude, in opposition to M. Vauquelin (55.), that the consumption of oxygen gas, by the respiration of snails, is not complete.

58. With regard to this partial consumption of oxygen gas by snails, it may be observed, that the author found the same thing sometimes to happen with slugs, which he, nevertheless, concludes generally to consume the whole of this portion of the atmosphere. The portion of oxygen gas unconsumed, is likewise so very small, and the result, by the test of phosphorus, so nearly approximates to that ob-

* *Memoirs on Respiration*, p. 146.

† *Ibid.* p. 150.

tained by Vauquelin, as to render it difficult to conceive, but for some occasional error, that they should not entirely coincide. Of the probable existence of such error, the great difference in the analysis, made by the combustion of phosphorus, and by the test of nitrous gas, furnishes strong suspicion: and it is known that this latter method requires many precautions to insure complete accuracy. Even if all these had been observed, more attention to the actual surrounding temperature was required, than seems, in these experiments, to have been bestowed; for, in a small graduated tube, air is itself the most sensible thermometer, and readily suffers a change of bulk from the slightest variations of temperature, and, in the measurement of small quantities, demands, therefore, the utmost attention and correctness. In his analysis of the air which had been respired by snails, M. Vauquelin could not well be deceived by the non-combustion of phosphorus; and, although this fact does not disprove the assertion of Spallanzani, yet, as it proves more, it is entitled to the preference; for, where the same degree of credit is attached, a positive must ever outweigh a negative proof. The snail too, in this experiment, might have died from some accidental cause, before the consumption of the oxygen gas was complete, while, in other instances, the same species of animal would entirely consume it: and we find, accordingly, that Spallanzani himself, in several parts of his work, asserts that snails, by their respiration, consume all the oxygen gas of the air*.

* Memoirs on Respiration, p. 153. 165. 172. 213. 222. 249. 340.

59. But not only does Spallanzani dissent from the conclusion of Vauquelin, as to the complete destruction of oxygen gas by the respiration of snails: he contends likewise, that a portion of the nitrogen gas at the same time disappears. He placed different single snails in several equal bulks of atmospheric air, where they remained till they ceased to exhibit any signs of life. To ascertain the consumption of nitrogen gas, he passed the residual air into the eudiometrical tube through mercury, and compared its bulk with that which it possessed before the snails were placed in it. Having then ascertained the complete destruction of the oxygen gas, by the test of phosphorus, he next withdrew the carbonic acid by means of lime-water; and every degree of diminution beyond $\frac{20}{100}$, which he considers as the proportion of oxygen gas contained in atmospheric air, was referred to the destruction of the nitrogenous portion of the air*.

60. But, in these experiments, he has overlooked many circumstances which ought to have been attended to: and hence the results differ so much as to render the conclusion quite unsatisfactory. In two instances, $\frac{20}{100}$ of the oxygen gas of the air disappeared, and from five to $\frac{8}{100}$ of the nitrogen: but, in two others, only $\frac{16}{100}$ or $\frac{18}{100}$ of the former were lost, and from three to $\frac{4}{100}$ of the latter†. Where, in one experiment, two snails were confined together, the air lost $\frac{20}{100}$ of its oxygen, but only $\frac{2}{100}$ of its ni-

* Memoirs on Respiration, p. 162.

† Ibid. p. 163.

trogen ; so that much less nitrogen gas was destroyed in this case by two snails, than in the former experiments by one. In other experiments with snails, he has more than once found the whole eighty parts of nitrogen gas remaining ; and the *helix vivipara* consumes all the oxygen gas of the air without producing any increase or diminution of the nitrogenous portion *. These facts, taken in connection with the apparently decisive experiments of Vauquelin, and those of the author himself with regard to slugs, lead us to the direct conclusion, that in the respiration of slugs, worms, and snails, the whole of the oxygenous portion of the atmosphere completely disappears ; that carbonic acid is, in all cases, produced, and that the nitrogen gas remains unaltered.

61. Nearly the same results were obtained by experiments on the respiration of another species of snails, whose residence is wholly in the water. Spallanzani found, that the *helix vivipara*, which inhabits still rivers and pools, consumed, by its respiration, the oxygen gas of the atmosphere, like snails which live on the land : that this action did not go on in temperatures a few degrees only above the freezing point, but was very considerable in higher temperatures : that water, deprived of its air by boiling, did not support life, neither did it, when freed from its oxygen gas, and standing in contact with nitrogen gas : that snails, confined to the bottom of a jar of water, consumed only half the quantity of air that those did which were allowed to come to the surface :

* Memoirs on Respiration, p. 214. 301.

that the water attracts oxygen gas to supply the place of that which is consumed; and that, when confined in air only, these snails, by means of their skins, consume all the oxygen gas, and produce carbonic acid, without changing the quantity of nitrogen gas*.

62. Other aquatic animals, as muscles (*mytilus edulis*) were next confined in a jar, and water was poured over them to the depth of an inch. They soon opened the thinnest part of their shell, and, in ten or fifteen minutes more, threw out a small quantity of water: the shells were then shut again for a few minutes, and this action was alternately repeated. When two of these muscles were placed in a tube, half filled with water and half with air, in a temperature of about 66° Fahrenheit, they continued to live seven days, and, on the eighth day, the oxygen gas of the air was considerably diminished. With a portion of oxygen gas occupying the superior part of the vessels, these muscles would live nine days, but when nitrogen gas was in the same way employed, they died in three days. In every case, whatever quantity of nitrogen gas was present, it always remained unchanged †. On several species of marine *testacea*, he likewise made experiments, and found that they consumed the oxygen gas of seawater, which attracts more to supply the waste; that when confined in air only, they consume all its oxygen gas, and that they soon perish when nitrogen gas only is made to rest on the water, producing no change upon it ‡.

* Memoirs on Respiration, p 288. 296. 301.

† Ibid. p. 304. 306. 309. ‡ Ibid. p. 311. et seq.

63. It has been already shewn (52.), that fishes cannot live in water deprived of air, nor unless this air be constantly renewed: and Dr Priestley proved, by experiment, that they deteriorate the air which water contains. Several minnows were confined by him in a large phial of water till they died: and the air being afterwards expelled from the water, was examined by the test of nitrous gas, and found to contain less oxygen than that in which a candle goes out. He likewise impregnated water, previously deprived of its oxygen, with nitrogen and with hydrogen gases, and found, that in such water, the fishes died in about an hour*. Mr Davy ascertained that fishes die in a few minutes in water containing nitrogen gas, but live in that which is impregnated with oxygen gas: that the proportion of this latter gas is diminished by them, and carbonic acid is produced; for, on adding lime-water to that in which the fishes had been confined, a cloudiness was very perceptible, indicating the formation of carbonate of lime†. Dr Carradori has observed, that fishes are able to exhaust water entirely of its oxygen, which ebullition is unable to effect: and that they die instantly in water wholly deprived of oxygen‡. To supply the waste of oxygen gas, occasioned by the respiration of fishes, water, as Scheele first remarked, is endued with the power of attracting it in pre-

* On Air, vol. v. p. 137. et seq.

† Beddoes's Contributions to Physical Knowledge, p. 137.

‡ Nicholson's Journal, vol. xvi. p. 76.

ference to common air, and nearly in the same proportion *. Dr Crawford observes, that if a portion of atmospheric air be exposed in an inverted jar to water which has had its air separated by boiling, the purer part will be attracted by the water, and the noxious portion will remain distinct in the vessel †. The experiments of Mr Dalton, however, do not allow us to suppose that water thus completely analyzes the air, for, according to him, if a quantity of water, deprived of air, be agitated with atmospheric air, the water will attract portions of each of its constituent parts the same as if they were presented to it separately in their proper density ‡. From the operation of this distinct attractive power of water for the two gases of which the atmosphere is composed, it follows, that the oxygen gas, contained in water, will bear a considerably larger proportion to the nitrogen gas than exists in the air of our atmosphere; and that as this oxygen gas is consumed by the respiration of aquatic animals, more will be attracted to supply its place, and to maintain the due respirability of the air.

64. The changes induced on the air by fishes are effected by the *branchiæ* or gills, which in form and structure vary greatly, according to the mode of life of the fish. In eels, and those fishes which live in impure water, the gills are supported by boney arches, and are very large in proportion to the size

* Thomson's Chemistry, vol. iv. p. 490.

† On Animal Heat, p. 145.

‡ Manchester Memoirs, vol. i. new series.

of the fish : others, as the lamprey, receive the water into their gills by the mouth, and expel it through several holes in their sides, while those fishes that move rapidly, and make long migrations, take in the water largely by the mouth, and reject it very often by the gills. The gills were by the ancients considered to perform an office for fishes similar to that which the lungs perform for land-animals ; and Mayhow conceived them to be especially constituted for separating the air from water, whereby some vital aërial property was conveyed into the mass of blood. Hence it is, he adds, that fishes alternately draw in and expel water, as land-animals receive and expire air *.

65. But a somewhat more particular description of the structure of the gills of fishes will much assist our conception of the manner in which the respiratory function is performed by them. In fishes, the heart consists only of one auricle and one ventricle ; and from the latter, one artery is sent off, which is spent entirely on the gills. This artery conveys venal blood from the heart, which, in its passage through the gills, assumes a florid hue, and being afterwards collected by the branchial veins into one large trunk, is distributed, without the intervention of a second heart, to all parts of the body ; from which it is again brought back to the heart in a venal state, to undergo the same circulation. The gills, upon which the branchial artery ramifies, are of great extent. In each side of the body of a skate,

* Tractat. Quinq. p. 259.

says Dr Monro, there are four double gills, or gills with two sides each, and one single gill ; or there are in all eighteen sides or surfaces on which the branchial artery is spread out. On each of these sides, there are about fifty divisions or doublings of the membrane of the gills ; and each division has on each side of it one hundred and sixty subdivisions or folds of its membrane, the length of which, in a very large skate, is about one-eighth part of an inch, and its breadth about one-sixteenth part : so that in the whole gills there are about 144,000 subdivisions or folds, the two sides of each of which are equal to the sixty-fourth part of a square inch ; or the surface of the whole gills in a large skate is equal to 2.250 square inches, that is, to more than fifteen square feet, which have been supposed equal to the whole external surface of the human body. When, after a good injection of the branchial artery, a microscope is applied, the whole extent of the membrane of the gills is seen covered with a beautiful network of exceedingly minute vessels ; and if distilled oil of turpentine, coloured with vermilion, has been injected with moderate force in a living or recently dead skate, some of the colourless parts of the oil exude upon the surface of the gills *. From all these facts, concerning the respiration of fishes, we learn, that the oxygen gas of the air contained in water, is changed (63.) into carbonic acid by the medium of their gills ; and that their blood, like that in the lungs of breathing animals, loses at the

* Monro on the Structure, &c. of Fishes, p. 15.

same time its venal characters, and becomes arterialized. Since also, it has been shewn (63.), that fishes do not live in water which contains only nitrogen gas, it is reasonable to infer, that, like water-snails and muscles, they produce no change upon it.

66. The amphibious class of animals, which live partly in air and partly in water, exhibits great variety in the structure of their circulating and respiratory organs. In the frog and toad, the heart consists of one auricle and one ventricle, as in fishes. The auricle receives the venal blood from the body, which passes into the ventricle, and from thence into the aorta; but this aorta soon divides into two branches, one for the body and one for the lungs; and hence but half of their blood is, in each circulation, exposed to the action of the air received into the lungs. In the turtle, the same intention is effected by a different mechanism; for, though the heart of that animal consists of four cavities, yet the ventricles freely communicate, and therefore the pulmonary artery and aorta arise in fact from one cavity; so that only half of the blood, thrown out at each contraction of the ventricles, will pass through the lungs, provided the areas of the two arteries correspond. As a part only of the blood is thus sent through the lungs in each circulation, it is plain that a cessation of the respiratory function does not necessarily put a stop to the circulation of the blood in these animals, as it does in fishes, in the mammalia and in birds; and such animals are said to possess a *pulmo arbitrarius*, or are able to live either in water or in air. The length of time which they can live without respiration, has been supposed to depend on the

structure of their lungs, and the capacity of these to receive and contain air. In the frog and toad, the lungs consist of two large membranous bags, divided into a great number of vesicles, over which the blood-vessels are minutely distributed; and, in the snake, viper, and many others, the lungs are continued down through the whole belly, in form of two bags. Many animals also of this class, as the otter and porpoise, whose lungs are constituted like those of man, can live a considerable time under water without breathing: and this power is much improved by habit. This is the case even in the human subject; for, while in ordinary persons, suffocation begins to take place in about half a minute when the body is submersed in water, those who dive for pearls, corals, &c. are said, by long practice, to be able to prolong this period to several minutes, being able to keep under water as long as the seal, porpoise, and the amphibia.

67. Of all the foregoing animals, therefore, which by naturalists have been placed in the class *amphibia*, none can be said to be truly amphibious, or to possess the faculty of supporting life, for an indefinite length of time, either in water or in air. This faculty belongs only to the syren, an animal said to be furnished both with lungs and gills. Something of the same sort may, indeed, be attributed to the frog at different periods of its existence; for we are told, that, during the first fourteen days of its life as a tadpole, it has only gills projecting like fins: that, by the thirty-sixth day, these are taken into the jaws, and form four rows of gills on each side, like those of fishes: and that, during this time, the lungs, as in the

foetus of the mammalia, are inert, and not called into use until the animal exchanges his watery habitation for an aërial one, when the gills gradually shrink *. Hence, then, it appears, that however various the structure of the lungs in this class of animals may be, a more or less constant supply of fresh air is required to enable them to support the functions of animal life.

68. To obtain a knowledge of the specific changes which the air suffers by the respiration of the amphibia, the following experiments were instituted. A toad, supported on a small hoop, was inclosed in one hundred and eight cubic inches of atmospheric air contained in a jar inverted over water, and standing in a room varying from 55° to 60° Fahrenheit. He died on the fifth day. The water had risen considerably into the jar, and the residual air was still farther diminished by agitation with lime-water, which it rendered turbid. Fifty parts, after being washed in lime-water, were next shaken in the eudiometer with the liquid sulphuret of potassa, and lost only one part of its bulk. The experiment was repeated by confining another toad, in the same manner, in another jar containing forty cubic inches of atmospheric air, inverted over mercury. Under the hoop which supported the animal, was placed a small cup containing 1.5 cubic inch of the water of potassa, which floated on the mercury. The whole was then set aside in a room, of the temperature of 64° . By the twelfth hour, the mercury had

* John Bell's Anatomy, vol. ii. p. 152.

risen nearly half an inch into the jar, which was thickly moistened with vapour, and the breathing of the animal seemed rather languid: by the twenty-first hour, he breathed very faintly; and, by the twenty-fourth hour, he had ceased to breathe. The jar was allowed to stand some hours, at the end of which time, the mercury stood about eight-tenths of an inch high, and one-tenth of an inch of fluid was deposited on its surface. The jar was now raised, and diluted sulphuric acid being poured into the alkaline solution, excited in it a very brisk effervescence. It is inferred, therefore, from these experiments, that the oxygenous portion of the air almost entirely disappears during the respiration of these animals, after which they cease to breathe; and that a large portion of carbonic acid is at the same time produced.

69. Proceeding on the supposition, that the loss in the bulk of air, evinced by the ascent of the mercury, in the last of the foregoing experiments, arose from the attraction of the carbonic acid by the alkaline solution, we endeavoured to ascertain the proportion which this loss of bulk bore to that of the whole air originally employed. With this view, a frog was procured, and placed in a jar of the capacity of forty cubic inches. Under the hoop which supported him, about half way up the jar, was placed a small cup, containing one cubic inch of the water of potassa, and the jar being then filled with atmospheric air, was inverted into a dish of mercury, and kept steady by a weight pressing upon it. In the room in which the animal was placed, the barometer stood at 29.2 inches, and the thermometer at 61°. At the end of twenty-nine hours, the animal was

resting quietly on the hoop, with no appearance of distress, and the mercury in the jar, when that in the dish was brought to a level with it, had risen six-tenths of an inch. In twenty-four hours more, the frog was still alive: his respiration seemed now to labour, and he rose often to the top of the jar as if desirous of escaping, or of obtaining fresh air: the mercury had now risen to 1.15 of an inch. From this time, the difficulty of breathing continued to increase, and, at the close of the fifty-ninth hour from the commencement of the experiment, after having lain quiet for a considerable time, he gave a convulsive struggle, and moved no more. The mercury in the dish was now brought to a level with that in the jar, and its height was 1.2 of an inch. The barometer, at this period, was 29.8, and the thermometer 65°.

70. In order to examine the residual air, we plunged the dish under water, which, rising into the jar, displaced the mercury, and the cup, with its solution, was then withdrawn under water. The residual air suffered no diminution by being shaken with lime-water, nor by contact with phosphorus, but it lost rather more than $\frac{1}{100}$ by agitation with the liquid sulphuret of potassa. The jar originally held forty cubic inches, but the animal, with the hoop, cup, and solution, occupied a space equal to four, so that the actual bulk of air employed was thirty-six cubic inches. Having placed the jar on its bottom, water, to the quantity of twenty-seven cubic inches, was poured in, till it reached the point to which the mercury, during the experiment, had risen; and this, therefore, indicated the volume of residual air: it

then required nine cubic inches more of water to fill the jar completely, which, consequently, was the bulk of air that had disappeared. Hence, therefore, we have $\frac{27 \times 29.8}{29.2} = 27.5547$, but $\frac{4 \times 27.554}{483} = .22819$ and $27.554 - .22819 = 27.32651$, the corrected volume of air at the close of the experiment. But farther, $36 - 27.32651 = 8.67349$, and $\frac{8.67349}{36} = \frac{1}{4.15}$; so that the diminution of bulk which the air suffered in this experiment is rather greater than $\frac{1}{4.54}$ the proportion of oxygen gas which the atmosphere contains. In a second experiment, another frog lived in the same volume of air about sixty hours, and the diminution which it suffered, after making the necessary reductions, amounted to $\frac{1}{4.868}$ of the whole. Where the carbonic acid, formed by the respiration of another frog, was suffered to remain, the jar, after the death of the animal, adhered firmly to the saucer in which it was inverted, and, when cautiously elevated, the surrounding mercury rushed in, and occupied only about one-tenth of the space which it filled in the above-mentioned cases. The inferences deducible from these facts, instruct us, that the diminution which atmospheric air suffers by the respiration of these animals, bears a near proportion to the oxygen gas which it contains, when all the carbonic acid is removed: and, as a small loss of bulk likewise takes place when this acid is allowed to remain, we must ascribe a part of the observed diminution to the necessary loss which always accompanies the conversion of oxygen gas into carbonic acid.

71. It follows from the preceding series of experiments, that the oxygenous portion of the air is changed by the respiration of amphibious animals in the same manner as by that of the other classes, carbonic acid, in proportion thereto, being, in all cases, produced: and that when the whole, or nearly the whole, of that gas is so changed, the animal no longer survives. But if the animal die when all the oxygen gas is changed, and all the air that has disappeared, when the carbonic acid is removed, be oxygen gas, then the bulk of air that remains, and is unchanged, must consist wholly of nitrogen gas; and, as this nitrogen gas, joined with the oxygen gas that has disappeared, makes up the whole bulk of air originally employed, it follows also, that, while the oxygen gas of the air has diminished and suffered change, the nitrogenous portion has continued undiminished and unaltered.

72. During all these changes operated on the air contained in water by the respiratory functions of aquatic animals, the water itself seems to suffer little or no alteration. Mr Carlisle took separate glasses, each containing one pound of distilled water, which was previously boiled to expel all its air, and then, inverting them over mercury, he put into them one gold fish, one frog, two leeches, and one fresh water muscle. The animals were confined several days in these situations, and exposed to the sun during January in temperature 43° and 48° Fahrenheit: but no air-bubbles were produced in the vessels, nor was there any sensible diminution of the water. The frog died on the third day, the fish on the fifth, the leeches on the eighth, and the muscle on

the thirteenth day. This experiment was made to ascertain the changes produced in water by the respiration of aquatic animals ; but the water had not undergone any chemical alteration *.

73. From this enumeration of the principal facts, concerning the changes induced on atmospheric air by the respiration of all these several classes of animals, we obtain positive evidence, that, in most cases (53. et seq.), its nitrogenous portion, as in the growth of vegetables (5. 29.), continues unaltered : and since, in the remaining cases, the air in degree is proved to suffer the same change, and the ultimate result, viz. the display of living action, is, in all respects, the same ; it is not, we hope, exposing ourselves to the charge of too hasty generalization, or of resting too much on analogy, if we conclude, that, in the whole view which we have hitherto taken of animal respiration, the nitrogen gas of the atmosphere remains unchanged. Moreover, as this gas itself suffers no change, so neither does it seem to exert any influence on the animal in contact with it ; for Spallanzani found, that snails could live in nitrogen gas twelve hours †, which is as long as they live *in vacuo* (51.) : and, in all the experiments we have made in atmospheric air, the animals did not appear to die from the superabundance of nitrogen gas, but from the small proportion, or total absence, of oxygen gas.

* Philosophical Transactions, 1805.

† Memoirs on Respiration, p. 347.

74. Assuming then, as a fact, that the nitrogen gas of the air neither produces nor suffers change, we have next to inquire, what becomes of the oxygen gas, which has been shewn, more or less, in all cases, to disappear. Is it *absorbed* by any organized structure of the animal adapted to the performance of such an office? No vessels fitted for such a purpose have been yet demonstrated in the animal system. The small size of the stigmata, or breathing pores, in insects, renders them but little suited to be receptacles for containing and decomposing air: and, in many of the *vermes* class, the mucous matter with which their bodies are constantly smeared over, must oppose great difficulties to such an absorption. In the case of aquatic animals, these difficulties are still farther increased; for the air must be first separated from the water before it can be taken up by absorption; and, after this is effected, it is not easy to conceive how the gills of fishes can be rendered capable of absorbing and emitting air. It is not probable, that this air is taken up in its entire state, for as the nitrogen gas undergoes no change (73.), its absorption can answer no obvious use, but would tend rather to impede the decomposition supposed to go on within the vessels, and the subsequent formation and emission of the carbonic acid. If, on the other hand, the air be considered to be decomposed previous to its absorption, then a new compound must be at once formed; and, if this be brought about by the union of some substance with the oxygen gas, then that gas, simply as such, cannot be held to enter into the animal system. No one has ever yet detected air in the animal fluids

while in a healthy state ; and if we consider the great extent of surface, and extreme minuteness of the vessels in the gills of fishes (65.), we cannot but consider them as well adapted to produce an extensive contact of surfaces, and but little fitted to absorb, decompose, and again emit aëriform fluids.

75. If to account for this supposed entrance of air into the vascular system, the agency of *chemical affinity* be had recourse to, by what means, we would ask, can its operation be in this case explained ? No sensible or obvious principle, equal to such an effect, can be held to reside in the blood, since the changes go on equally in all these animals, though the blood be of various colours, and, in many instances, where it is totally devoid of colour. During a torpid state also, Spallanzani has shewn, that no change is effected by the animal on the air * ; and consequently, no oxygen gas is then attracted by the blood, although, if the supposed carbon of that fluid be considered to attract this gas, the union ought still to proceed, because, according to the received opinions, the animal system is at this period surcharged with carbon. Neither can the conditions, indispensable to the operation of chemical affinity, be in these cases fulfilled ; for the interposition of organized substance between the air and the blood, altogether precludes that degree of absolute contact, which is held to be essential to chemical action. Even if the oxygen gas were attracted into the blood by the operation of chemical affinity, by

* Memoirs on Respiration, p. 334.

what power would the carbonic acid that is formed be again given out by that fluid? No chemical agent either in the air or the water can be imagined equal to the re-attraction of it through the organized structure of the animal: nor is it conceivable, how the blood, by any power of its own, should be able to emit it, independent of such agent. And to suppose, that, by a power of chemical affinity, this gas should enter into the blood, and be afterwards expelled from it, as carbonic acid, by any method analogous to the ordinary animal excretions, is too inconsistent to be entertained for a single moment.

76. On the grounds, therefore, that the oxygen gas of the air does not obtain admission into the blood-vessels, either by the function of absorption, or by the operation of chemical affinity, we must reject the belief of its union with the supposed carbon of the blood, to form the carbonic acid that is produced. Still, however, the gradual disappearance of that gas, and the production of carbonic acid which ensues, justify the conclusion, that in the animal, as well as in the vegetable, kingdom, they observe always a regular and progressive ratio, and are, in fact, proportional to each other, which admits of no other solution than that of their being converted into one another. To effect this conversion, however, no other substance but the animal was present, in these experiments, from which the carbon could be derived; consequently, the acid must be formed by the union of carbon furnished by the animal with the oxygen gas of the air, and this, too, exterior to the vascular structure of the animal.

77. Those who maintain, that the carbonic acid is not directly formed by the union of the oxygen gas of the air with the animal carbon, but that it escapes ready formed from the animal system, ought to point out some other source from whence, in sufficient quantity, the oxygen gas can be derived : to tell us at the same time what becomes of the oxygenous portion of the air that actually disappears : and why the production of carbonic acid bears always so constant a proportion to the loss of this oxygen gas. To suppose with Spallanzani, that this acid is yielded by the process of digestion, because some snails which had been well-fed, furnished more of it than others which had suffered a long abstinence, is by no means proving the point ; for a snail which had long fasted, yielded as much, in one instance, as those which had been recently fed, and, in the other examples, the starved snails fell short only in a small degree. Every animal function, also, is, *cæteris paribus*, carried on best in a state of health and vigour, which again depends altogether on a due supply of food : hence, therefore, the debility succeeding to abstinence, must affect the organs of respiration, in common with the other organs, and consequently their power of acting so completely on the air. Whatever substances, moreover, are received into the animal system, suffer or produce some change : but to suppose, that carbonic acid should be first formed in the stomach, then taken up by the lacteal vessels, and carried through the mass of blood to be again thrown out by the respiratory function, simply as carbonic acid, is not only without proof, but against all probability. It is also to the quantity of air

changed, and not to that of food taken in, that this acid bears a proportion ; and, provided living action be equally well maintained, as much air seems to be required by an animal when he abstains from food as when he takes it, and as much carbonic acid to be produced. In as much, however, as a long abstinence from food, debilitates the system, and affects the production of carbon, in so much will it diminish the quantity of carbonic acid, which the animal is accustomed to form.

78. But the quantity of this acid, when formed by the respiration of a given volume of air, does not seem to exert any noxious operation on the animal powers ; for Spallanzani found, that animals placed in a given bulk of air, did not live longer when the carbonic acid was abstracted by an alkaline solution as soon as formed, than when it was suffered to remain * ; and, in all our experiments, where the carbonic acid was allowed to remain, the death of the animals seems to have arisen, not from the over-proportion of that acid, but from the diminished quantity, or total absence, of the oxygen gas. Since, indeed, air is necessary to the continuance of living action in all animals, and its nitrogenous portion appears to suffer no alteration, this necessity must arise from its containing oxygen gas, and from the requisite changes which, in respiration, that gas is made to undergo. When, therefore, the greater part, or the whole of the oxygen gas of the air is so changed, death ought to happen ; and then accordingly,

* Memoirs on Respiration, p. 317.

and not till then, does this event take place. It is therefore to the small proportion or total absence of oxygen gas, and not to the presence of carbonic acid formed out of that gas, that the cessation of the animal functions, in all the foregoing examples, is to be immediately ascribed.

body is obedient to these general laws; for all the solid matter of animals has been once in a fluid state, and, having passed into the solid form, becomes a recipient for other fluids, one of which the solids

varied *.

80. This conversion of fluid into solid matter cannot, however, be effected in a living animal body without the constant presence of heat, the agency of which

OF THE CHANGES INDUCED ON THE AIR BY THE RESPIRATION OF BIRDS, OF QUADRUPEDS, AND OF MAN.

SECTION I.

79. NOT only, as we have seen, is water necessary to prepare the organization of vegetables, and of the inferior animals, for exhibiting living action, but it is required also by those which belong to the superior orders. “The whole material world,” says Mr Hunter, “has been very properly divided into solids and fluids, these being the only essentially different states of matter which we are able to observe. From one of these states into the other, matter appears to be continually passing; but no species of matter can assume a solid form without having first been in a fluid state; neither can any change take place in a solid, till it be first reduced to, or suspended in, a fluid. The living animal

body is obedient to these general laws ; for all the solid matter of animals has been once in a fluid state, and, having passed into the solid form, becomes a recipient for other fluids, out of which the solids themselves may, in turn, be increased and renovated *.”

80. This conversion of fluid into solid matter, cannot, however, go on in the animal body without the constant presence of heat, the agency of which is essentially necessary to carry forward these transformations. When, to a certain degree, heat is abstracted from the body, its vital functions gradually decline, and at length finally cease. Many animals experience these effects periodically, without injury to the vital organs, and the actions of life re-appear as the temperature of the season returns. In our own climate, the hedge-hog, the bat, the dormouse, and several birds, pass into a state of torpidity during the winter season ; and, in the more northern parts of Europe and America, the bear and alligator do the same : from which we may conclude, that a certain degree of heat is necessary to sustain the actions of life in the superior, as well as in the inferior animals.

81. The operation of light on the colour of animal bodies (50.), and probably on some of their other properties, has been already noticed. In the human subject, the colour of the skin depends on that of the reticular membrane placed beneath the cuticle, which assumes various colours in different parts of

* Treatise on the Blood, p. 12.

the earth. In the native American, the inhabitant of Asia, and the southern European, the colour varies from dark copper to pale tawny ; and in the negro it is quite black. The inhabitants of the northern countries, on the other hand, are white : and, not only are those parts of the body, which are most covered, the whitest, but a sensible difference in colour exists in the same person at different seasons of the year. These variations in colour have been held to depend very much on the agency of light.

82. The necessity of atmospheric air to the continuance of living action in all the superior animals, was long ago proved by the experiments of Mr Boyle, soon after the discovery of the air-pump ; and he observed farther, that the function of respiration is quickly suspended, unless the lungs are furnished with a regular supply of fresh air *. “ Animals, whose hearts have two ventricles and no *foramen ovale*, says Mr Derham, as birds, dogs, cats, and mice, die under the action of the air-pump in less than half a minute, counting from the very first exsuction, especially in a small receiver †.” In corroboration of these facts, we may mention an experiment, exhibited before the Royal Society, by the celebrated Dr Hooke. He cut away the ribs, diaphragm, and pericardium of a dog, whereby the lungs and heart were brought into view ; and then, dividing the windpipe, he introduced into it the nozzle of a pair of double bellows, and made, at the

* Boyle's Works, vol. i. p. 99.

† Physico-Theology, p. 8.

same time, several small punctures through the outer coat of the lungs. By blowing in a stream of fresh air, which continued to escape through the small apertures made in the lungs, he was enabled to keep those organs fully distended. As long as he supplied the lungs with air, the actions of life continued, and the heart beat very regularly ; but, on intermitting the supply, the dog would immediately fall into dying convulsive fits, and revive again as soon as the lungs were filled with a stream of fresh air. The circulation through the lungs continued both during their distended and collapsed state, and as well when they were kept at rest, as during a state of motion : whence he concluded, that neither the motion of the lungs, nor the cessation of their motion, nor the stopping of the circulation of the blood through them, was the immediate cause of death, but the want of a sufficient supply of fresh air *. By the researches of later philosophers, it has been proved, that the air, in all animals, serves the same uses ; to fit it for which, it undergoes the same changes. In our inquiry into the nature of these changes, the manner in which they are effected, and the uses which they are found to serve, we shall confine ourselves chiefly to the facts which take place in human respiration, not only on account of their greater interest, but also because they have occupied more research, and, having been more frequently submitted to experiment, are in some respects better ascertained.

* Lowthorpe's Abrid. Phil. Trans. vol. iii. p. 66.

83. When atmospheric air is respired by man and by other animals, it undergoes two remarkable changes: its bulk is diminished, and its qualities are altered. This diminution of bulk was early noticed by Boyle, who estimated it at about $\frac{1}{30}$ of the air employed. Mayhow, whose genius enabled him to anticipate so many important discoveries of modern chemistry, confined an animal in a glass-vessel inverted over water, and, by the aid of a syphon, brought the water on the inside of the vessel to a level with that on its outside. Having then marked the height of the water by pieces of paper affixed to the vessel, he observed its gradual rise as the animal continued to breathe: and then comparing the space occupied by the air at the commencement of the experiment, with that which it possessed when the animal ceased to breathe, he found that it was reduced about $\frac{1}{14}$ part of its bulk *. In the experiments of Dr Hales, the degree of diminution varied from $\frac{1}{13}$ to $\frac{1}{30}$ of the whole air employed †: and in those of M. Lavoisier from $\frac{1}{31}$ to $\frac{1}{60}$ part ‡; with which the results of Dr Goodwyn's experiments on his own respiration nearly coincide ||. Dr Priestley confined a mouse in a jar containing a given quantity of air, which was inverted over mercury: the animal was suffered to remain two or three days after he had died, in which time there was no sensible diminution of the air, but

* Tractat. Quinq. p. 104.

† Statical Essays, vol. i. p. 238.—vol. ii. p. 320.

‡ Mem. Acad. 1777 and 1780.

|| Connexion of Life with Respiration, p. 51.

on passing lime-water into the jar, the air was diminished $\frac{1}{28}$ part of its bulk; and when, in a subsequent experiment, the residual air was agitated in water, it was reduced between one-fifth and one-sixth of the whole*. Dr Crawford found also, that when the experiment was made over mercury, the diminution was not sensible; but that, if water of potassa was added to the residual air, it became mild, and the air was diminished in the same degree as if the experiment had been made over water, or nearly one-fifth of its bulk†. These variations in the results arise, no doubt, from the more or less complete attraction of the carbonic acid by the fluids over which the experiments were made; and, from the whole of them, we may collect, that, when mercury is employed, which has no attraction for carbonic acid, the diminution is hardly sensible; but that when this acid is completely abstracted by an alkaline fluid, the loss of bulk amounts nearly to one-fifth of the whole air employed. This inference corresponds very exactly with the facts which occur in vegetation, and in the respiration of the inferior animals.

84. But experiments of this nature, although they shew the extent to which the destruction of the oxygen gas, contained in a given quantity of air, may, by the process of respiration, be made to proceed, yet they do not apply to the ordinary circumstances in which that function is carried on; for the air of

* Observations on Air, vol. v. p. 112. et seq.

† On Animal Heat, p. 146.

the vessels in which the animals were confined, must, by repeated breathing, have become less and less fit for respiration, and was therefore gradually declining from that state in which it is usually inspired. We have seen, that several of the inferior animals will live in a given quantity of air until its oxygen gas is completely (54. et seq.) consumed: but those of the superior orders do not bear this total privation. Birds die in air confined by lime-water, before they have consumed two-thirds of its oxygen gas: and a mouse and guinea pig expire when about three-fourths of this gas have disappeared, although the carbonic acid be withdrawn*. Spallanzani observes likewise, that birds and quadrupeds consume not more than $\frac{19}{100}$ of the oxygen gas of the air, and sometimes only 17, 16, or $\frac{15}{100}$, and then die, even although the carbonic acid be removed†. Lavoisier found, that by repeatedly withdrawing the animal from the vessel when he began to sicken, and re-introducing him after he had revived, he could be made to consume almost the whole of the oxygenous portion of the air‡. In all the foregoing experiments, it may be doubted, whether the same actual diminution in bulk takes place, as would have occurred if the same volume of air had been submitted to successive respirations in the open atmosphere. It is only with the latter

* Higgins's Minutes of a Society, p. 158.

† Memoirs on Respiration, p. 318.

‡ Mem. Acad. 1783.

kind of diminution, viz. that which takes place in natural respiration, that we are at present concerned; and as this cannot be determined by experiments made on brutes, we must resort to the facts which have been ascertained by those instituted on the respiration of man.

85. A knowledge, however, of the diminution of bulk which the air, during respiration, suffers, implies a previous determination of the quantity ordinarily inspired. To ascertain this point, many modes of experiment have been adopted, and the conclusions which have been drawn from them very widely differ. Borelli estimated the bulk of air taken in at a single inspiration, at 15 cubic inches *; Mr Kite from 12 to 17 †; Dr Goodwyn at 14 ‡; Mr Davy from 13 to 17 §; and Drs Jurin, Hales, Haller and Sauvages, at 40 cubic inches. With the conclusion of these latter authors the experiments of Dr Menzies nearly coincide, and as the methods which he adopted seem less liable to objection than those of any other author, it may not be improper shortly to give the detail of them. He procured an allantoid, and fixed to it a machine consisting of two pretty large tubes, joined at right angles, nearly in the form of a common brass cock. One end of the horizontal tube was connected with the allantoid, and the other received into the mouth, while the upright tube, which rose from its centre, communi-

* De Motu Animal.

† Essay on Apparent Death, p. 24.

‡ Connexion of Life with Respiration, p. 28. et seq.

§ Researches, p. 410. & 433.

cated with the atmosphere. The tubes were large, and valves, made out of an allantoid, were affixed to the end of the upright tube, and to that attached to the allantoid, so that the air, when expelled from the lungs, should not escape into the atmosphere, nor return from the allantoid, after having once entered it. Precautions were taken also, by covering the mouth and nostrils, to prevent any air from passing in or out of the lungs, except by the tubes above mentioned. Things being thus prepared, he began to respire, and did not remove his mouth from the tube till he had filled the allantoid, taking care to stop his nostrils during expiration. The allantoid was filled, in repeated trials, by about 56 expirations, as natural as possible; and as its capacity was 2400 cubic inches, the average bulk of air thrown out of the lungs by each expiration, was 42.8 cubic inches. He then fixed another allantoid, whose capacity had been previously ascertained, to the end of the upright tube; and having filled it with atmospheric air, he inspired the air from one allantoid and expired it into the other, and the quantities were found to be nearly the same. Several persons of the middle size repeated this experiment with nearly the same result; the difference being scarcely ever more than one or two cubic inches. By another mode of experiment, first proposed by Boerhaave, of plunging a man into a tub of water up to his chin, and judging of the dilatation of the lungs from the ascent and descent of the water, he obtained, by several trials, nearly the same results; and when these same men were made to breathe from and into the allantoids, in the manner above described, the cor-

respondence by the two methods was almost complete *. As there seems no obvious source of inaccuracy in the processes here employed, and their results so remarkably coincide ; and as they present the average bulk deduced from 56 respirations, we may conclude, says Dr Bostock †, that 40 cubic inches is the quantity of air employed in an ordinary act of respiration ‡.

86. The difficulty in arriving, by experiment, at certain conclusions respecting the volume of air taken into the lungs in each inspiration, may arise from a difference in the state or capacity of those organs in different individuals ; from the relative vigour or debility of the muscular powers carrying on the respiratory function ; from the circumstances in which the animal is placed ; the composition of the air itself ; or the manner in which it is breathed. In many modes of experiment also, the friction between the air and apparatus employed, or the resistance which this latter may create to the ordinary process, will greatly vary the result : and considerable errors must likewise have arisen from the variation in bulk, occasioned by the change of temperature, which the air, during its respiration, suffers ; from the difficulty of breathing in a natural manner when the

* Menzies on Respiration, p. 21. et seq.

† On Respiration, p. 34.

‡ Besides the respectable authorities mentioned in the text, Dr Bostock quotes the names of Blumenbach, Chaptal, Bell, Fontana, and Richerand, as estimating the bulk of a single inspiration at between 30 and 40 cubic inches of air.

mind is directing that process ; and from the embarrassments opposed to the natural action of the respiratory organs by the contrivances adapted to them.

87. It will not be denied, that the size and capacity of the chest must, in a certain degree, regulate the quantity of air which is taken into or expelled from it ; and since respiration is neither wholly a voluntary nor an involuntary act, but within certain limits partakes of the nature of both, and is carried on by the exertion of muscular powers, the bulk of respired air must vary also, either from an alteration in the action of these powers, or from a change in the will of the agent who exerts them. This may be illustrated by considering the different quantities of air taken into the lungs in different states of natural and forced respiration. Dr Goodwyn, supposing a person at death to make a *complete* expiration, endeavoured to ascertain the bulk of air then remaining in the lungs, which he estimated at 109 cubic inches *. This estimate he formed by measuring the capacity of the chest, in subjects who had died a natural death by disease, previous to which the expiratory powers must have been much weakened, and unable, in consequence, to expel so much air as when in a state of health and vigour ; and in such cases, therefore, expiration might be final without being complete. Mr Cruickshank observes, accordingly, that the lungs in the dead body, (though expiration is the last action of life), always retain more

* Connexion of Life with Respiration, p. 27.

air than is given out at several expirations*. By a very different mode of experiment, we find Mr Davy to conclude that his lungs, after a forced expiration, contain only 32 cubic inches of air, when it is reduced to the temperature of 55° , but which, by the heat of the lungs, and saturation with moisture, are increased to 41 cubic inches; and, after a natural expiration, they contained 118 cubic inches†; so that the difference between the two states of natural and forced expiration is 77, which is somewhat more than Dr Menzies allows, who remarked that many men, after an ordinary expiration, could still expel from their lungs 70 cubic inches of air‡. Mr Davy adds, that his estimate of 118 cubic inches, as the capacity of the lungs after natural expiration, agrees very well with that of Dr Goodwyn, who makes it about 109‡; and, on the supposition that the general debility which precedes the ordinary extinction of life, so weakens the expiratory muscles, as to disable them from making so complete an expulsion of the air, as they can effect when in health and vigour, the agreement is very striking; for nearly the same quantity of air would, in that case, remain in the lungs at the period of natural death, as after that of ordinary expiration.

88. Dr Bostock conceives, that Dr Goodwyn's estimate of 109 cubic inches of air remaining in the lungs after complete expiration, is not very remote

* On Insensible Perspiration, p. 97.

† Researches, p. 409. & 410.

‡ Dissertation on Respiration, p. 31. § Ibid. p. 411.

from the truth : and he objects to Mr Davy's mode of ascertaining the residual air of the lungs after a forced expiration, from a supposition that the hydrogen gas which he inspired for that purpose was not, in consequence of its low specific gravity, uniformly diffused through all the cavities of the lungs : and therefore, that the proportions of the gas discharged could furnish no accurate estimate of those which were retained *. But Mr Dalton has shewn, that hydrogen gas and atmospheric air intermix, when the former is kept in a phial above the latter, and communicating only by the small tube of a tobacco-pipe ; and both in a state of rest † : How much more readily then may this be expected to take place, where the gases are exposed to so large a surface, such great agitation, and increased temperature, as they must have been in the experiments of Mr Davy. Neither is the small quantity of air, which Mr Davy assigns, so incompatible, as Dr Bostock supposes, with the anatomical structure of the thorax ; for if we call to mind the space which the heart and the lungs occupy, and recollect, that, under a violent exertion, the chest is made to contract in every direction, and more especially by the ascent of the diaphragm nearly to the fourth or fifth rib, there is no difficulty in imagining the quantity of air in the lungs, in such circumstances, to be nearly that which Mr Davy's experiments assign.

* Essay on Respiration, pp. 17. 25.

† Manchester Memoirs, vol. i. new series.

89. From a review, therefore, of all the facts and experiments above stated, we venture to draw the following conclusions, as approaching nearest to the truth. First, then, according to Mr Davy, the lungs contain, after a forced expiration, a bulk of air equal to about 41 cubic inches; and according to the same author and Dr Goodwyn, they contain, after a natural expiration, from 109 to 118 cubic inches: therefore the state of forced is to that of natural expiration as 41 to 118. Secondly, according to Dr Menzies, 40 cubic inches of air are received into the lungs at each ordinary inspiration: therefore the state of natural expiration to that of natural inspiration will be as 118 to 158. Mr Davy found likewise, that by a forced expiration after a forced inspiration, he could expel from his lungs 190 cubic inches of air, and Dr Menzies often found it to amount to 200 inches: therefore the state of greatest exhaustion of the lungs is to that of greatest repletion, as 41 to 231. But the 41 cubic inches of air, when inspired at temperature 55° , occupied a bulk equal only to 32; and therefore, by the same rule of proportion, 190 cubic inches, inspired at the same temperature, will be increased to 241.5: consequently, the greatest diminution of the capacity of the chest to its greatest expansion will be as 41 to 241, in the case of Mr Davy. But these numbers must be considered as indicating proportions only, the absolute quantities being different in different persons*. These facts decidedly shew how much

* At the time of making these experiments, Mr Davy states

the volume of air in the lungs will at all times depend on the relative capacity of those organs, on the more or less vigorous state of the expiratory powers, and on the degree of voluntary exertion with which the function may be performed.

90. The circumstances in which the animal may happen to be placed, will render this variation still more striking. Thus, from the experiments of Mr Kite and Mr Coleman, we learn, that in the act of drowning, animals are able to expel almost all the air which their lungs contain, by which those organs are brought into a state of collapse *. Dr Goodwyn, on the other hand, found, that in three executed persons, the lungs were expanded almost to their utmost extent, containing 250, 262, and 272 cubic inches of air †: and Mr Coleman observes, that when, previous to their suspension, he secured the trachea of animals by a ligature at the instant an inspiration was made, in less than four minutes they ceased to struggle, though the whole of the air was confined within the lungs, and no obstruction to the passage of the blood existed from their collapse ‡. Dr Baillie also has often observed the lungs filling the chest, and distended with air and mucus, in persons who have died asthmatic: so that to die and to

his chest to have been narrow, not exceeding in circumference 29 inches. (Researches, p. 410.).

* Kite on Apparent Death, p. 27. 29.

Coleman on Suspended Respiration, p. 7. et seq.

† Goodwyn's Essay, p. 25.

‡ On Suspended Respiration, p. 111—138.

expire are by no means synonymous terms,—an observation long since made by Mayhow, who remarked, that if air be drawn into the lungs, and the mouth and nostrils afterwards closed, “*quamvis inflati maneant pulmones, mori tamen necesse erit, quia non licet expirare* *.” If indeed we reflect, that during submersion in water no fresh air can enter into the lungs, but that all which they contain may freely escape; and if we consider, that before suspension by the neck in the human subject, a deep inspiration, under the influence of fear, as Dr Goodwyn observes, is made, and that no air can afterwards pass out, if the cord completely close up the trachea; it is reasonable to expect, that this variation in the bulk of air contained in the lungs should obtain, under the very different circumstances in which respiration is brought to a stand.

91. How much the composition of the air itself, and the manner in which it is breathed, will vary the bulk of residual air in the lungs, we may collect from the experiments of various authors. Dr Hales moistened a bladder, and fixed to it a fosset, both of which would contain 74 cubic inches of air. Having blown up the bladder, he put the small end of the fosset into his mouth, and, at the same time, pinched his nostrils close, that no air might escape through them, and he then breathed to and fro the air contained in the bladder. In less than half a minute, he found a considerable difficulty of breathing, and was forced after that to draw his breath very

* Tractat. Quinque, p. 300.

fast : and at the end of the experiment, the suffocating uneasiness was so great as to oblige him to take away the bladder from his mouth. Towards the end of the minute, the bladder was become so flaccid that he could not blow it above half full, with the greatest expiration that he could make *. When also Mr Davy respired atmospheric air in a natural manner, he took in, he says, only 13 cubic inches and expelled 12.7, so that only about $\frac{1}{43}$ part of the original bulk was retained : when he made one respiration of 100 cubic inches of air, the diminution was to 99, or $\frac{1}{100}$: when, after a complete exhaustion of his lungs, he respired 141 cubic inches of air, once only for one-fourth of a minute, they were reduced to 139, or $\frac{1}{70}$ nearly : and when 161 cubic inches were breathed for about a minute, their bulk was diminished to 152, or $\frac{1}{18}$ † ;—in every case, the diminution augmenting with the repetition of the respiration, and consequent impurity of the air, and distress of the respiratory organs. So likewise, when Dr Henderson breathed from and into the gasometer 600 cubic inches of air for four minutes, they were reduced to 570, or lost $\frac{1}{20}$ of their bulk ; and he adds, that he held on respiring until the sense of oppression about the chest obliged him to desist ‡. These distressing symptoms, brought on by the repeated breathing of the same quantity

* Statical Essays, vol. i. p. 238.

† Researches, p. 431, 432, 433. 435.

‡ Nicholson's Journal, May 1804.

of air, were felt in a still greater degree by Mr Kite; for on respiring 591 cubic inches of atmospheric air from and into a bladder, he experienced, in one minute, great anxiety at the breast, which in half a minute more became intolerable: his face swelled, became black, and felt excessively hot, and sparks of fire danced before him: loss of sight, giddiness and confusion of the senses succeeded, and at the end of little more than two minutes, he fell back into a chair. He was relieved by fresh air, but remained confused and giddy*. The amount of the diminution of respired air, says Professor Pfaff, depends not only on the *time* during which a given volume of air is respired, but principally on the magnitude of the volume of air itself: it must be proportionally less the greater the quantity inspired. He breathed 144 cubic inches of air once only in the time of ten or twelve seconds, and the diminution was four cubic inches, or $\frac{1}{36}$ of the primitive volume: when he respired the same volume of air twice, during twenty seconds, it lost eight cubic inches, or $\frac{1}{18}$: and when it was thrice respired, during thirty seconds, the diminution amounted to twelve cubic inches, or $\frac{1}{12}$ of the primitive volume†. Now, in all these cases, the volume of air respired was precisely the same, and could not, therefore, affect the ratio of diminution: but as the times were doubled and tripled, so nearly were the degrees of diminution. But the more frequently the same air

* Essay on Apparent Death, p. 25.

† Nicholson's Journal, December 1805.

is breathed, the more unfit does it become for respiration : and to this change of composition, more than to the time, or the magnitude of the volume of air, is the increased degree of diminution to be ascribed.

92. This will perhaps appear more striking, if we attend to what happens in respiring nitrous oxide, which is composed of the same elements as atmospheric air, but contains a much larger proportion of oxygen. After exhausting his lungs, Mr Davy inspired 108 cubic inches of this gas, which, when expired, were reduced to 99, or had lost $\frac{1}{12}$ of their bulk. When he made two respirations of the same quantity of the oxide, the diminution was to 95, or about one-eighth : and when he respired 102 cubic inches of nitrous oxide, mixed with $\frac{1}{50}$ of common air, for half a minute, the volume of air, after the seventh expiration, was reduced to 62, or had suffered a loss equal to $\frac{1}{2.55}$ *. Hence it appears, that in the natural respiration of atmospheric air, only a small diminution (85.) of its bulk takes place : that this diminution increases as the air becomes vitiated (91.) by repeated respirations, or is breathed in a preternatural manner : and that when a gas of the same elementary materials, but combined in very different proportions, is substituted into the place of pure atmospheric air, the diminution increases in a tenfold degree. Now, the repeated breathing of the same atmospheric air, has been shewn to bring on

* Researches, p. 394. 416.

the most distressful symptoms, and at length an utter inability to continue respiration ; and Mr Davy tells us, that, after a voluntary exhaustion of his lungs, he could respire the nitrous oxide with accuracy, when stooping, for about half a minute, but, even then, strong sensations were produced, with fulness about the head rather alarming : that if the respiration extended to three-fourths of a minute, he could not rely on the accuracy of any experiment ; and that the determination of blood to the head became, in less than a minute, so great, as often to deprive him of voluntary power over the muscles of his mouth *. But respiration is a function carried on by the exertion of muscular powers, in a great degree obedient to the will (87.) ; and the quantity of residual air in the lungs in preternatural respiration, will, at all times, be much influenced by the manner in which the will exerts itself, and the degree in which the muscles are able to act. When, therefore, the power of the will over the muscles is in any degree diminished, or is wholly lost, or the muscles themselves are much weakened, a proportional derangement will take place in the respiratory function ; and as, in the natural condition of the body, expiration is subsequent to inspiration, the ability to inspire will last longer than the ability to expire : consequently, the cessation of the process is brought about by a failure in the expiratory powers. But if the expiratory powers are unable to expel the air from the lungs, it must remain in

* Researches, p. 392.

those organs ; and hence we see, in all the foregoing examples, that the diminution in the volume of expired air was greater in proportion as the respiratory organs suffered distress or oppression, and amounted even to more than one-third of the air inspired when all voluntary powers ceased.

93. By Mr Davy, however, and many others, the difference in bulk between the volume of nitrous oxide inspired, and that which is expired, is considered to arise in every case from a “ rapid absorption of this elastic fluid by venal blood through the moist coats of the pulmonary veins * :” and he also thinks it “ reasonable to suppose, that the whole compound atmospheric air, passing through the moist coats of the vessels, is first dissolved by the serum of the venal blood, and, in its condensed state, decomposed by the affinity of the red particles for its oxygen ; the greater part of the nitrogen being liberated unaltered, but a minute portion of it possibly remaining condensed in the serum and coagulable lymph, and passing with them into the left chamber of the heart †.” It happens, rather unfortunately for this opinion, that, in the natural respiration of atmospheric air, a very small difference exists between the inspired and expired volumes, though the powers of absorption, if such there be, must then be acting in their greatest vigour ; while under an almost total exhaustion of muscular and vital power, this absorption is considered to take place in an extraordinary degree. But the subject

* Researches, p. 396.

Ibid. p. 447.

of the absorption of elastic fluids in the human lungs is of so much importance in itself, and has so much divided the opinions of physiologists, as to demand from us a more distinct and detailed discussion.

94. When then the air, received into our lungs, is said to obtain admission into the blood-vessels by a process of absorption, it may be proper, in the first place, to inquire into the structure of those organs, at least so far as to ascertain by what means such a process takes place. The lungs, one of which occupies each cavity of the chest, are composed principally of air-cells and blood-vessels, connected through their whole extent by intervening cellular membrane. The *trachea*, or windpipe, on its arrival in the chest, divides into a right and a left branch, which branches again subdivide into smaller ones called *bronchia*, and these into others still more minute, until at length they lose their cartilaginous texture, become membranous, and expand into a cellular structure, which fills at all times the cavity of the chest. “The cells composing this structure, are purely membranous, of an irregular figure, compressed and closely connected, and have a free communication with each other. Between the different lobes, lobules, and cells of the lungs, a large quantity of common cellular substance, destitute of fat, is interposed, which unites and strengthens them; but the cells have no communication with this substance; for, when air is blown into it, the lobules are compressed, but when the air is blown in through a branch of the *trachea*, the cells are again distended, and the lobules recover their former dimen-

sions *.” The pulmonary artery, which conveys the venal blood from the right side of the heart, divides into two branches, which are dispersed through the substance of the lungs; and its smaller branches, running in the common cellular substance, become at length inconceivably minute, forming at last a *plexus*, or fine net-work, upon the proper cells: and they terminate afterwards, partly into exhalent vessels, and partly into corresponding branches of the pulmonary veins. These veins, by frequent anastomosis, diminishing in number and increasing in size, form at last four large trunks, which finally deliver the blood into the left side of the heart. From this description, it is manifest, that, between the air contained in the cells, and the blood flowing through the vessels, are interposed the coats both of the cells and vessels. When, therefore, air is said to enter into the blood from the cells of the lungs, it must, in some way, be conveyed through the coats of these cells and blood-vessels. After what manner, therefore, is it able to effect a passage?

95. Every anatomist will allow, that the surface of the cells of the lungs, like every other surface of the body, is duly furnished with absorbent vessels, of which not only the ordinary absorption of fluids carried on by this surface, but the frequent removals of morbid matter from the bronchial cells, supply abundant proof. Mr Cruickshank has frequently seen the absorbents of the lungs turgid with blood in cases of hæmoptoë, which blood they had ab-

* Fyfe's Compendium of Anatomy, vol. ii. p. 148.

sorbed from the air-cells instead of their transparent fluid *. Does the air also, which is supposed to pass out of the cells of the lungs into the blood-vessels by a process of *absorption*, take the route of these absorbent vessels? To this question we reply, in the language of Haller, that the fineness of those vessels, the mucus perpetually smearing the surface of the cells, the elastic nature of air itself, and its repulsion by water, so that it neither penetrates moist paper, cloth, nor skin,—all demonstrate that no air by this route gets into the blood †. If, indeed, air were taken in by the absorbents, it must, as Dr Goodwyn observes, take the route of those vessels, and, by passing directly to the right side of the heart, change the colour of the blood there; which, however, does not happen ‡: nor, when air was forced down the windpipe of a dog, in the experiments of Dr Hales, was it able to pass into the pulmonary artery or veins ||.

96. If, then, no proof exist of the passage of air into the blood by the ordinary course of the absorbent vessels, the only other mode of effecting this purpose that has been hitherto suggested, is the power of *chemical affinity*. What then are the chemical affinities subsisting between venal blood and atmospheric air? About the middle of the 17th century, Dr Lower observed, that the upper surface of

* On the Absorbents, p. 42.

† Prim. Lineæ, par. 306.

‡ Connexion of Life with Respiration, p. 62.

|| Statical Essays, vol. ii. p. 276.

venal blood, received into a vessel, acquired a scarlet colour by exposure to the air : that if this surface was removed, the subjacent one was soon changed to the same colour : that if the cake of blood, after being allowed to settle in the vessel, was inverted, its exterior and upper surface speedily also assumed a florid hue : and, lastly, that if venal blood was shaken in a vessel, so that the air thoroughly intermixed with it, it became entirely florid *. These opinions were afterwards held by Sig. Fracassati and Dr Slare, the latter of whom observes, that the blood thrown up by a rupture of the capillary vessels of the lungs, is frothy and of a scarlet colour ; the first of which effects he attributes to the intermixture of air, and the latter to its tinging power †. Mr Hewson employed similar arguments to prove, that the florid colour, acquired by venal blood on exposure, was produced by the contact of the air : and, by injecting air into the jugular vein of a rabbit, he found that it there also rendered the blood florid ‡. M. Cigna not only confirmed the foregoing facts, but proved also that the change of colour in this fluid did not take place when the blood was covered with oil or placed *in vacuo* ; and Dr Priestley ascertained, that not only by common air, but more especially by oxygen gas, this florid colour was produced on the black crassamentum of blood ||.

* Tract. de Corde, p. 178.—An. 1669.

† Lowthorpe's Abrid. Phil. Trans. vol. iii. p. 235.

‡ Hewson on the Blood, p. 9.

|| Priestley on Air, vol. iii. p. 66.

97. In effecting these remarkable alterations in the colour of the blood, the air itself, at the same time, suffers material changes. Dr Priestley found, that in twenty-four hours oxygen gas was so far depraved by being in contact with venal blood, that one measure of it and two of nitrous gas occupied the space of a measure and a half, whereas, at the beginning of the experiment, they occupied the space of no more than half a measure *. Dr Goodwyn confined venal blood under a jar of oxygen gas inverted in mercury, and repeatedly observed that the change of colour was always very sudden, and, after several minutes, the mercury ascended two or three lines; from which he concluded that a small portion of the air had disappeared †. The precise change, however, which the air underwent, seems first to have been observed by Dr Girtanner, who placed six ounces of venal blood in a jar of oxygen gas inverted in mercury: the blood presently assumed a florid colour: the air was somewhat diminished in bulk, and contained a portion of carbonic acid, which was attracted by lime-water ‡. Dr Bostock observes also, that a diminution of oxygen and production of carbonic acid take place when a piece of crassamentum is placed in a jar filled with oxygen gas ||. The same production of carbonic acid we found to occur when blood is placed in contact

* Priestley on Air, vol. iii. p. 75.

† Goodwyn's Essay, p. 61.

‡ Memoirs on Irritability in Beddoes' Obs. on Calculus, &c. p. 219.

|| On Respiration, p. 227.

with atmospheric air. A quantity of this fluid was received into a cup, and confined in a jar of air inverted in water, a glass of lime-water having been previously placed in the cup. The internal surface of the jar was soon bedewed with moisture, and a pellicle began to form on the lime-water, which in a few hours was increased to a thick crust of carbonate of lime. The crassamentum was then removed, and a fresh glass of lime-water was placed in the serum, which in thirty-six hours had acquired a crust like the former, and the water had risen considerably into the jar. In another experiment, where the serum was placed for twenty-four hours in a jar of air inverted in mercury, the residual air rendered lime-water milky, and the remainder had lost a part of its oxygen. A similar production of carbonic acid seems to have occurred, when, with a small diminution of the gas, a slight change of colour was produced on venal blood by placing it in contact with nitrous oxide, in the experiments of Mr Davy : for when a solution of strontian was admitted to the oxide, it became slightly clouded, and, with the diminution of bulk that followed, minute portions of carbonic acid and nitrogen gas were produced *. Hence then we learn, that when venal blood is exposed to the contact of atmospheric air, of oxygen gas, or of nitrous oxide, it presently assumes a florid colour, and, at the same time, the volume of air is somewhat diminished, and a portion of carbonic acid is produced.

* Researches, p. 377, 380, 387.

98. Does then the carbonic acid, which is here met with, proceed ready formed from the blood, or is it in part formed by the decomposition of the air? No one has yet proved that any aëriform fluid, much less that carbonic acid, exists naturally in the blood; and if this be true, no such aërial acid can be expected to issue from it. The carbonic acid also, is not formed by blood when it is confined in nitrogen gas; neither does the colour of the blood, in that case, undergo any sensible change: but this acid is formed by blood, either in oxygen gas, in nitrous oxide, or in atmospheric air, all of which are deteriorated thereby; whence it follows, that without the presence of oxygen gas, the blood is unable to form carbonic acid, and that this acid, therefore, is, in part, formed out of that gas. If the oxygen gas that disappears do not contribute to form the carbonic acid that is produced, in what other manner can its loss be accounted for? or from what other source than the oxygen gas of the air, in contact with the blood, can that ingredient of the acid be derived? Those who suppose the carbonic acid to be furnished by the blood, independent of the air employed, must likewise suppose that the nitrogen gas is furnished by it also; for the experiments of Mr Davy teach us, that a portion of that gas, as well as of carbonic acid, is always present when nitrous oxide is decomposed, which renders it probable that the same thing likewise occurs when air is changed by venal blood. But in what manner the blood should be able to furnish nitrogen gas, it is not easy to conceive, since no affinity exists between that gas and

venal blood *. We infer, therefore, from these facts, that atmospheric air is decomposed by being placed in contact with venal blood, its oxygenous portion being in part converted into carbonic acid, and a quantity of its nitrogen being, in consequence, left free.

99. But, supposing the air to be thus decomposed by the blood, it still remains a question, whether it has been first attracted by that fluid, then decomposed, and afterwards in part expelled ; or, whether the decomposition has been effected without such previous attraction and intermixture of air. The only evidence of this supposed attraction seems to be the small diminution of bulk which the air in all cases suffers ; but this cannot be considered as a proof of the attraction of the air ; for it is a necessary consequence of that conversion of oxygen gas into carbonic acid which has been shewn (11.) to take place when these substances are brought into contact. Even granting to the blood this power of attracting air, or its oxygenous portion, it is not easy to conceive why it should so readily lose it, and again give out this air in the form of carbonic acid. No change of quality in the blood, nor any variation of temperature, can have taken place sufficient to alter so rapidly its affinity for these substances : and it cannot proceed from a want of affinity between the blood and the carbonic acid that is formed ; for that acid suffers a greater diminution, either than oxygen gas or atmospheric air, by being placed in contact with

* Davy's Researches, p. 375.

blood. We incline, therefore, to the opinion, that neither the air nor its oxygen gas is attracted by, and diffused through the blood, as happens with several gases when placed in contact with certain fluids: but that the air is decomposed, and its oxygen gas changed into carbonic acid, without entering into the substance of that fluid.

100. But, for the formation of this acid, the blood must supply carbon, since no other substance was present from which it could be derived: and it is well known also, that carbon enters largely into the composition of that fluid; and our experiments (97.) prove, that it exists as well in the serous as in the more solid parts. By some it may be objected, that because carbonic acid is formed directly by the combustion of charcoal, it cannot be produced at so low a temperature as exists in these experiments. To this we can reply only by an appeal to the general facts exhibited through the whole course of our inquiry, by which it appears, that both by the living functions of vegetables and animals, and by the decomposition of animal and vegetable matter, this acid is, in like manner, formed at temperatures equally low. Even those who consider this acid to have proceeded ready formed from the blood, cannot attribute its production to the operation of heat; for in the animal body, the temperature of the blood seldom exceeds 100° —a degree of heat incompetent to form carbonic acid by any process analogous to combustion. The combination of many bodies is, indeed, greatly accelerated by being exposed to very high temperatures; but this surely does not set aside the fact of their spontaneous union at temperatures

much more low. From this review of the effects which take place between the blood and air, we conclude, that the chemical phenomena which arise when these substances are placed in contact, do not prove an attraction and diffusion of air through the blood ; but shew only that a reciprocal action takes place, by which a new product is formed : no inference, therefore, in favour of an attraction of air by the blood in the lungs, can be drawn from the reciprocal action which they exert on each other out of the body.

101. To the operation of chemical affinity also, a degree of absolute contact is required, which may, and does exist between air and venal blood out of the body ; but the intervention of the coats of the cells and blood-vessels altogether forbids this necessary condition in the lungs. The supposition, that the coats of these vessels and cells are so thin, that, when moist, they allow the air, or its oxygen gas, to pervade them, is wholly gratuitous, and in opposition to the results (94.) of direct experiment : and the belief, that certain pores exist, through which elastic fluids may permeate, is equally unsupported by anatomical fact ; for the terminations of the absorbent and exhalent vessels are the only orifices which are known to open on the surface of the bronchial cells. If, indeed, air did permeate the bronchial cells through any supposed pores, it would more readily pass into the cellular substance which connects them together than into the pulmonary vessels, and thus would create, at all times, an emphysematous state of those organs ; but this is never known to be the case, for these cells are impermeable by air. It is, lastly, ex-

tremely difficult to conceive how the same air, which is so readily confined in membranous substances out of the body, should, with such perfect freedom, pass to and fro through a much more complicated structure within it. It has indeed been said, that, when any gas is confined in a bladder, it will permeate its coats and escape, while atmospheric air, passing at the same time through these coats, will supply its place. Allowing this to be the case, it will not be denied, that many days, or even weeks, are required to accomplish this operation : and it bears, therefore, no sort of analogy to that rapid attraction and expulsion of air which is supposed to go on through the cells and blood-vessels of the lungs. It should be remembered also, that the bladder, when removed from the body, soon loses its living properties, by which its power of resisting the passage of fluids may be diminished ; for we know that the bile, a much denser fluid than air, is during life perfectly retained within the gall bladder ; but a short time after death, its colouring matter often escapes, and gives to the surrounding viscera a yellow tinge. Neither can any thing, necessarily residing in the venal blood, be held sufficient to account for this supposed attraction of air ; for Girtanner found, that arterial blood produced the same changes *, and the like occur (97.) when serum only is employed ; and even if, by superior affinity, the blood did attract air through the coats of the cells and vessels of the lungs, in what way shall we account for its so ra-

* Memoirs on Irritability, p. 228. 231.

pidly losing this superiority, and again giving out nearly the whole of this air, through these same blood-vessels and cells?

102. But if, either by the function of absorption, or by the operation of chemical affinity, air did enter into the blood, we may surely with reason demand some proof of its presence; yet, says Haller, "*Nulla unquam in vivo calido animali bulla aëris in sanguine visa est* *." This opinion is confirmed by the direct experiments of Dr D. Darwin; for having inclosed a portion of the jugular vein of a sheep between two ligatures, it was cut out, stripped of its adhering cellular membrane, and then thrown into a glass of water of temperature 100°, standing under the receiver of an air-pump. It at once sank to the bottom, and did not rise when the air was exhausted; nor, when afterwards taken out, wiped dry, and laid on the floor of the receiver, did it exhibit any swelling under the exhaustion of the vessel. The experiment was repeated with a similar result on a portion of the vena cava of a swine †.

103. Neither do the effects resulting from the admixture of aëriform fluids with the blood, favour the notion of the entrance of air into that fluid. "*Animal, cui aër in sanguinem inflatur,*" says Haller, "*perit certo et velociter; neque quidquam satis certi est in sanguinis venarum pulmonalium aucto rubore* ‡." This assertion is confirmed likewise by

* Prim. Lin. par. 306.

† Philosophical Transactions, vol. lxiv. p. 345.

‡ Prim. Lineæ, loc. cit.

direct experiment. When Dr Girtanner injected oxygen gas into the jugular vein of a dog, he cried dreadfully, breathed quick, and died in three minutes: when nitrogen gas was thrown in, death happened in 20 seconds*. Air, says M. Bichat, thrown into the vascular system, quickly brings on agitation, convulsions, and death†. By forcing air through the windpipe into the lungs with a syringe, and confining it there, he has made it to enter into the blood-vessels, which immediately brings on agitation and exertion in the animal; and if an artery in the leg or foot be now opened, the blood will spring out frothy, and full of bubbles of air. If hydrogen gas has been used, the bubbles may be inflamed; and when this frothy blood has flowed 30 seconds, the actions of life cease, and cannot again be restored, even although fresh air be supplied‡.

104. As the chief arguments in favour of the entrance of air into the blood, have been drawn from the experiments of Mr Davy in his excellent “*Researches*” into the nature and respiration of nitrous oxide, it may, perhaps, be required that we should notice the leading proofs which he adduces in favour of it. Mr D. had found that hydrogen gas, when inspired, only mingles with the air present in the lungs, and is again thrown out unaltered with a portion of the residual air||. The bulk of this residual air, reduced to the temperature of 55°, he es-

* *Memoirs on Irritability*, p. 221. 223.

† *Recherches sur la Vie et la Mort*, p. 179. ‡ *Ibid.* p. 303.

|| *Researches*, p. 399.

timates at 32 cubic inches, being composed of nitrogen 23, carbonic acid 4.1, and oxygen gas 4.9 *. When he breathed the nitrous oxide, the proportions of these residual gases appeared nearly the same as when hydrogen gas was respired ; which led him to conclude, that no portion of the nitrogenous or oxygenous gases was produced by the decomposition of the oxide in the lungs †. But a reference to the results of Mr Davy's experiments will shew, that the carbonic acid in the lungs and air-holder, which, before breathing the oxide, was estimated at 4.1, was afterwards increased to 5.2 cubic inches ; and that the oxygen gas also was increased from 5.6 to 6.1. In another experiment, the carbonic acid was increased, after breathing the oxide, from 4.1 to 6.8, and the oxygen gas, at the same time, from 5.5 to 6.3 ; and Mr Davy admits, " that the quantity of carbonic acid and oxygen is rather greater than that which existed in the experiments on hydrogen †." Finding, indeed, the bulk of these two gases in the lungs and air-holder to be only 9 cubic inches when hydrogen was respired, and that it amounted to 13.1 when nitrous oxide was breathed, and, denying that the acid is formed by the immediate decomposition of the nitrous oxide itself, Mr Davy is led to believe, that " it is wholly or partially liberated from the venal blood through the moist coats of the vessels § ;"—a supposition, against which it has, we trust, been already sufficiently argued.

* Researches, p. 409.

† Ibid. p. 414.

‡ Ibid. p. 413. 415.

§ Ibid. p. 420. 422.

105. Together with this increase of oxygen gas and carbonic acid, beyond what the residual air in the lungs would supply, there was a considerable increase also of nitrogen gas, which corresponds with what takes place when this oxide is exposed to blood out of the body ; for, besides the carbonic acid produced, there are always present small portions of nitrogen gas *. Since, therefore, the same products are obtained when this oxide is breathed, as when it is exposed to blood out of the body, it may be inferred that they are effected by similar means. These means cannot, in the latter case, be absorption in the sense we apply that term to the living human body ; because the blood speedily becomes an inert mass, bearing no analogy in its properties to the absorbent function in the lungs : and, even if chemical attraction were allowed to operate between this oxide and blood out of the body, this will not apply to the circumstances in which they are respectively present in the lungs ; because the intervention of cells and blood-vessels wholly forbids that degree of actual contact, which is essential to chemical action. It is therefore only on the supposition of a *decomposition* taking place, that these contradictory results can be reconciled.

106. Against the idea of such decomposition in the lungs, Mr Davy urges, that “ it is difficult to suppose how nitrous oxide, which requires the temperature of ignition for its decomposition by the most inflammable bodies, should be partially ab-

* Researches, p. 387.

sorbed, and partially decomposed at 98° , by a fluid possessed apparently of uniform attractions *." To this it may be replied, that the difficulty of decomposing a substance by some bodies, in certain circumstances, at a high temperature, is no proof that it cannot be decomposed, by other bodies, in other circumstances, at a low temperature. This oxide, Mr Davy believes, to be in some manner decomposed by the blood during its circulation with that fluid: but, does the temperature of the blood exceed that of the lungs? Even out of the body, and in the ordinary temperature of the atmosphere, the experiments of Mr Davy (97.) evince, that this oxide can be partially decomposed by venal blood.

107. According to the earliest and the latest experiments of M. Lavoisier on the respiration of atmospheric air, its nitrogen gas was considered to be in no respect affected by that function; in which conclusion Goodwyn, Menzies, and most other authors, have acquiesced: and we have seen that this gas is not affected by the growth of vegetables, (5. 29.), nor by the respiration (73.) of the inferior animals. Dr Priestley, however, at one time supposed, that the oxygen gas passing the membrane of the lungs, carried with it some part of the nitrogen with which it was previously combined; but, on the suggestion of Sir Charles Blagden, he afterwards thought it more probable that the deficiency of nitrogen gas was owing to the greater proportion of it existing in the lungs after the process than before †.

* Researches, p. 415.

† Experiments on Air abridged, vol. iii. p. 380.

Mr Davy, from the results of his experiments, has been led to revive this opinion concerning the entrance of nitrogen into the blood. In the respiration of atmospheric air, he calculates that 5.2 cubic inches of nitrogen disappear every minute* : and when nitrous oxide is breathed, he supposes that “an immense quantity of this substance is taken into the blood ; and that the part of it not expended in new combinations, during living action, is liberated in the aëriform state by the exhalents, or through the moist coats of the veins †.” But why the blood should so much more powerfully attract nitrogen from nitrous oxide than from atmospheric air, is enough of itself to beget doubts of the truth of Mr Davy’s conclusion, more especially if we consider, that “nitrogen and oxygen exist, perhaps, in this oxide in the most intimate union which those substances are capable of assuming ‡.” The only direct evidence in favour of this opinion, is the loss in bulk which nitrous oxide suffers in respiration. For this we have endeavoured to assign an adequate cause (91. et seq.); and a reference to Mr Davy’s experiments (92.) will shew, that when he breathed this oxide once only, the loss in bulk was one-twelfth ; when twice, the loss was about one-eighth ; and when seven times, the loss amounted to more than one-third ; thus regularly increasing with the repeated respiration of a noxious gas, and consequent debility of the expiratory powers, just as takes place in the repeated breathing of the same volume of atmospheric air.

* Researches, p. 434.

† Ibid. p. 415.

‡ Ibid. p. 328.

108. Mr Davy, however, taking for granted that immense quantities of nitrogen gas enter into the blood, observes, that “this being true, the quantity of nitrogen produced in respiration, ought to be increased in proportion as a greater quantity of nitrous oxide enters into combination with the blood *.” This he attempts to prove by experiment. Having exhausted his lungs, he inspired the oxide out of a silk bag, containing eight quarts of that gas, and thus made nine respirations. The gas of the first expiration was not preserved, but that of the second gave 29 of the oxide, and 17 parts nitrogen gas; the third was as 22 to 8; the fifth, as 27 to 6; the seventh, as 23 to 7; and the ninth was as 26 to 11. So far, therefore, from the nitrogen gas increasing in proportion to the supposed combination of the oxide with the blood, it observes nearly a decreasing ratio; and Mr Davy accordingly admits, that the “results of these experiments are not so conclusive as could be wished †.” He nevertheless goes on to say, that “if any portion of nitrous oxide were decomposed immediately by the red particles of the blood, one should conjecture that the quantity of nitrogen produced, ought to be greater during the first inspirations, before these particles became fully combined with condensed oxygen ‡.” And what is the fact from the experiments just related? Why, that in the second expiration, the nitrogen gas was 17 parts; in the third only 8; and in the fifth but 6; evidently showing that the greater

* Researches, p. 416. † Ibid. p. 418. ‡ Ibid. p. 419.

quantity of this gas did appear during the first inspirations ; and not after the supposed combination of oxygen with the red particles of the blood.

109. Mr Davy has breathed the nitrous oxide, in a state of purity, for four minutes and a half, and some have respired it five minutes ; and he states the proportion of oxide absorbed to be as great in the last, as in the first inspirations, the consumption, by the same individual, being nearly in the ratio of the time of respiration *. He thinks about a pint, or 30 cubic inches, to be the ordinary range of consumption in different individuals, which, he says, is not far from two cubic inches, or about one grain every second ; or in one minute 120 cubic inches, or 60 grains †. This quantity amounts, in five minutes, to 600 cubic inches, or 300 grains ; and in one hour, to 7200 cubic inches, or 3600 grains ; equal to more than 31 gallons measure, or seven ounces and a half troy weight, if it were possible so long to continue respiring, and the consumption were nearly in the ratio of the time of respiration. But taking the proportion only for five minutes, and allowing all that Mr Davy would deduct as liberated again from the blood in the same space of time, it would amount to a bulk and weight altogether inconsistent with what we know to be the state and condition of the sanguiferous system ; and the inference, if followed out, might therefore, in the form of a *reductio ad absurdum*, be employed against Mr Davy's opinion.

* Researches, p. 425.

† Ibid. p. 427.

110. The idea, that “the absorption of nitrous oxide by venal blood, is owing to a simple solution of the gas in that fluid, analogous to its solution in water or alkohol*, will by no means obviate the objection to such immense quantities of it entering into the blood; for it appears, that but a very small diminution of bulk takes place (96.) when this oxide is placed in actual contact with venal blood; and when again it “is carried through the pulmonary veins and left chamber of the heart to the arteries,” and made to undergo decomposition, in order that “its oxygen may be chiefly expended in living action,” and its “nitrogen be partially consumed in new combinations†, will not then the oxygen gas be in contact with a fluid, over which, out of the body, it does not “perceptibly diminish‡,” and the nitrogen gas exist in one over which it “possesses no power of action, and with which it is incapable of combining||?” The effects resulting from such admixtures of aëriform fluids with the blood in the living body, have already (103.) been distinctly noticed. If it be said, that this gas and atmospheric air are received into the blood in a peculiarly modified state as to their gaseous form, then they cannot be supposed to pass entire (93.) through the moist coats of the cells and vessels, but must suffer a previous decomposition in the lungs. If, on the other hand, they do pass entire into that fluid, and if, from its incapacity of combining with the blood,

* Researches, p. 378.

† Ibid. p. 419.

‡ Ibid. p. 381.

|| Ibid. p. 375.

“ the greater part of the nitrogen gas (93.) be again liberated unaltered,” why cannot we discover some traces (102.) of its existence in that fluid ? or why should its presence be so rapidly fatal, as experiment (103.) proves it to be ?

111. But the supposed stimulant effects of this oxide have been assigned as an argument for its being in some way contained in the blood. In many cases, however, these effects do not follow the inhalation of that gas : and, in other instances, common air, breathed under the impression of its being this oxide, has produced them. Similar effects likewise are attributed by some authors to the inhalation of carbonic acid ; and Dr Percival quotes M. Beaumé as relating the history of a man who was recovered from apparent death, produced by exposure to the foul air of a cellar, who asserted that he had felt neither pain nor oppression, but that, at the point of time when he was losing his senses, he experienced a delightful kind of delirium. This account, adds Dr Percival, receives confirmation from the testimony of Dr Heberden, who says, that he had seen an instance in which the fumes of charcoal brought on the same delirium as intoxicating vegetables produce. The Abbé Fontana likewise breathed a certain portion of hydrogen gas, not only without inconvenience, but with unusual pleasure : he had a facility in dilating the breast, and never felt an equally agreeable sensation, even when he inhaled the purest oxygen gas *. From these facts, it ap-

* Manchester Memoirs, vol. ii. p. 491, 492.

pears, that the inhalation of any gas, unfit to carry on respiration, will, in some persons, produce effects precisely similar to those which have been ascribed to an absorption of nitrous oxide ; and, on the other hand, that no such stimulant effects are produced in other persons by the inhalation and supposed absorption of that gas. In no respect, therefore, can we trust to the effects, which succeed to the respiration of this oxide, as affording any decisive proof of its absorption and commixture with the blood.

112. It is, as we have seen, the opinion of Mr Davy, and of some other authors, that air passes entire into the blood-vessels, and is decomposed during the circulation of that fluid. Now, the gases expired are, and must be admitted to be, formed out of those previously inspired, and, in natural breathing, the air suffers but a small diminution (85.) in bulk ; and the time of an ordinary inspiration occupies from $\frac{1}{15}$ to $\frac{1}{25}$ part of a minute. To suppose the inspired air to enter through the moist coats of the cells and blood-vessels, suffer decomposition in the blood, and be again returned through these vessels and cells, with so small a difference in bulk, and in so short a time, seems not only without proof, but against all probability, especially if we consider the extreme minuteness of the pulmonary vessels, and the great rapidity with which the blood is transmitted through them. If, indeed, the reciprocal changes which take place between the blood and the air were effected during the circulation of that fluid, and it were only after that period the expired airs could be rendered, we ought not to expect them for nearly three minutes ; for suppose one ounce and a

half of blood to be propelled at each contraction of the heart, and each contraction to occupy $\frac{1}{72}$ of a minute, and the whole circulating mass to be about 324 ounces, then a complete circulation is performed in the time of 216 pulsations, that is, in three minutes. The almost instantaneous return of the inspired airs, however, is totally adverse to this fact : and if this be true of one respiration, it must be true of all, since the power acting, and the substances acted upon, preserve always the same relative state and circumstances, and the actions are performed not in time only, but in succession also ; and thus, as one cannot begin until the other is ended, each is only a repetition of the same event, which, being operated by like powers, and in like circumstances, must afford invariably a like result.

113. If then there be no proof that air, either by absorption, or by chemical affinity, enters into the blood-vessels of the lungs, the sensible effects which it is there known to produce on the blood, must, in some other way, be accounted for. The celebrated Lower, not only first remarked the scarlet colour which black blood acquired by exposure to the air, but he first proved likewise, that the same change was produced by the air received into the lungs. In combating the opinion of Willis, that the florid colour of the blood was derived from an innate fire kept up in the heart, he asserted, that, if this were the case, it should, from their similarity of structure, be effected by the right, as well as by the left, side of the heart, whereas the blood sent into the lungs from the right heart was dark and venal. He passed a ligature also round the *trachea* of an animal, so

as effectually to exclude the air, and then opening an artery in the neck, he found that the blood which issued from it was black : and he farther remarked, that in a strangulated animal, where the entrance of air was precluded, the blood in the left side of the heart was like to venal blood ; but if, before it coagulated, it was forced through the lungs, and air was at the same time blown into those organs, it again became florid, after the death of the animal, in the same manner as when he was alive *. Mr Hunter, by means of a double bellows, was enabled by one action to exhaust the lungs of their air, and by another to pass, at the same time, fresh air into those organs, without mixing the two together ; and having introduced the nozzle of his bellows into the *trachea* of a dog, whose heart and lungs had been previously brought into view, he found, that, while he continued the artificial breathing, the heart continued to act as before, but more frequently : if, however, he stopped the motion of the bellows, the contractions of the heart gradually became weaker, and at length ceased. On recurring to the artificial breathing, the heart again began to move, and became at length as strong as at first. This process he repeated ten times, stopping from five to ten minutes between each ; and he observed, that every time he left off working the bellows, the heart became extremely turgid with blood, and that the blood in the left side was as dark as that in the right, which was not the case when the bellows were

* Tractat. de Corde, p. 184.

working *. Dr Goodwyn repeated these experiments, both with common air and with oxygen gas, on dogs, and on lizards and toads, whose lungs consist only of a transparent bladder; and he observed the same changes of colour, according as the lungs were supplied with or deprived of fresh air †. This suddenness of effect seems wholly inconsistent with the supposed entrance of air into the blood, and its subsequent decomposition during the circulation of that fluid; and favours the probability of its being actually decomposed in the lungs.

114. Not only, however, in the lungs, but in the bronchial cells of those organs does this decomposition appear to happen, of which the vast extent of their surface is a strong presumptive proof. Dr Hales estimates the capacity of each cell, in the lungs of a calf, at the $\frac{1}{100}$ part of an inch diameter, and the surface of all the cells together at 40,000 square inches, to which, if 1635 square inches be added as the estimated surface of the bronchia, the sum of the surface of the whole lungs in that animal will be 41635 square inches, equal to nineteen times the surface of a man's body, which is computed at fifteen square feet ‡. Dr Keil estimates the diameter of each cell in the human lungs at $\frac{1}{50}$ part of an inch, and its superficies at 0.01256: the whole number of cells he calculates at 17,441,860, which, multiplied by the superficies of each, makes the sum

* Observations on the Animal Economy, 2d edit. p. 134.

† Goodwyn's Essay, p. 58. 62.

‡ Statical Essays, vol. i. p. 241.

of the superficies of all the cells, independent of that of the bronchia, equal to 21907 square inches, or more than ten times greater than that of the whole body *. The irregularity in the shape and size of the cells, and the want of adequate means correctly to determine their number, render an accurate estimate of their entire surface extremely difficult, if not altogether impossible: it is sufficient for our purpose to have shewn, that they exceed, in extent, by many times, the surface of the human body. The ramifications of the pulmonary artery and veins, running over these cells, must be minute and extensive, in proportion: and “by this admirable contrivance,” says Dr Hales, “the blood is spread out into a vast expanse, commensurate to a very large surface of air, from which it is divided by thin partitions, so very thin as thereby to bring the blood and air within the sphere of each other’s attraction †.” Such a structure, how favourable soever it may be to an extensive contact of surfaces, is but little fitted to promote a mixture of substances by absorption.

SECTION II.

115. In the former section, we have endeavoured to shew the necessity of atmospheric air to the continuance of living action in the superior animals,

* Tentam. Med. Phys. p. 80.

† Statical Essays, vol. i. p. 247.

and have noticed the diminution of bulk which it suffers in respiration : we have attempted to ascertain the volume of air ordinarily inspired by man, and to appreciate the relative proportions which exist in the lungs in the different states of those organs : we have laboured to prove, that the actual bulk of air in the lungs must, and does vary, according to the various conditions of the system, to the circumstances in which respiration is carried on, the composition of the air itself, and the mode in which it is breathed ; deducing from the whole, that the bulk of residual air in the lungs, at any period of life or of death, must be calculated always with reference to these considerations. We have spoken generally against the entrance of any part of the inspired air into the blood-vessels of the lungs, by the function of *absorption*, as unsupported by anatomical fact ; and have held its supposed *attraction* by the blood through the coats of those vessels as inconsistent with the acknowledged laws of chemical affinity : we have denied that any proof exists of air being, at any time, present in healthy blood, and have demonstrated its fatal effects whenever it gains admission into that fluid : we have examined the experiments and arguments of Mr Davy, in favour of an absorption of aëriform fluids by the blood, and have found them wholly insufficient, and leading to conclusions which are completely at variance with the established laws of the animal system. Lastly, we have adduced arguments against the possibility of the inspired air being decomposed during the circulation of the blood ; and have assigned reasons in proof of its actual decomposition in the lungs, and even in the

bronchial cells of those organs. This leads us, therefore, in the next place, to speak of the nature and extent of this decomposition ; in other words, of the changes in quality which the air of our atmosphere experiences in the lungs during its respiration.

116. It has been already shewn (96. et seq.), that when the dark crassamentum of venal blood is placed in contact with atmospheric air, or with oxygen gas, it speedily assumes a florid colour : that the bulk of air is, in a small degree, diminished : that a portion of the oxygen disappears, and a quantity of carbonic acid is produced : and these effects have all been considered to arise from the combination of the oxygenous portion of the air with carbon supplied by the blood. In the living body, the venal blood experiences the same change of colour (113.) when atmospheric air, or oxygen gas, is received into the lungs : the air itself also is somewhat diminished in bulk : a portion of its oxygen gas, as we shall hereafter see, disappears : and that carbonic acid is, in like manner, produced, was first proved by the experiments of Dr Black. “ So early as the year 1757,” says this distinguished philosopher, “ I convinced myself that the change produced on wholesome air by breathing it, consists chiefly, if not solely, in the *conversion of part of it into fixed air* : for I found, that by blowing through a pipe into lime-water, or a solution of caustic alkali, the lime was precipitated, and the alkali was rendered mild *.” At a later period, Mr Bewley detect-

* Black's Lectures by Robison, vol. ii. p. 87.

ed the formation of carbonic acid in respiration by a method somewhat similar : he found, that on breathing through an infusion of litmus, the same change to a red colour was produced in it, as when it was exposed to the action of fixed air ; and when, by adding a few drops of the water of potassa, the blue colour was restored to the infusion, it could again be made to dissappear by supersaturating it with the acid expired from the lungs *.

117. The particular substance which constituted the wholesome part of atmospheric air, was not, however, known to Dr Black at the time his experiments were made : and long before the compound nature of the atmosphere was ascertained, it had been supposed by many philosophers, that, to use the language of Bishop Berkeley, “ there was no such thing as a pure simple element of air. There is,” he adds, “ some one quality or ingredient in the air on which life more immediately and principally depends. What that is, though men are not agreed, yet it is agreed it must be the same thing that supports the vital and the common flame ; it being found, that when air, by often breathing in it, is become unfit for the one, it will no longer serve for the other. This quality of the air is necessary both to vegetables and animals, whether terrestrial or aquatic ; neither beasts, insects, birds, nor fishes, being able to subsist without air : and when air is deprived of this ingredient, it becometh unfit to maintain either life or flame, even

* Priestley on Air, vol. v. p. 383.

though it should retain its elasticity *.” Dr Hooke asserted, that this ingredient or substance inherent in, and mixed with the air, is like, if not the very same with that which is fixed in saltpetre, by which, during combustion, inflammable bodies are dissolved †. The same opinion was afterwards held by Willis, Lower, and Mayhow, all of whom likewise considered the nitrous quality of the air to act an important part in respiration. The last author, in particular, made experiments precisely similar to those which have lately been brought forward, to prove, that both by the burning of a candle and other combustible bodies, and by the respiration of animals, the nitro-aërial particles of the air were exhausted, whereby the volume of air was diminished, and the residual air was unable afterwards to support either life or flame ‡. The exhibition, however, of this peculiar, or nitro-aërial, part of the air in a distinct and separate form, we owe to the genius of Dr Priestley, who, by the discovery of pure or dephlogisticated air, in the month of August 1774, first made us acquainted with the true composition of the atmosphere. This great discovery, and the use which he made of it, enabled this celebrated philosopher to propose the first consistent explanation of the phenomena of respiration that had ever been offered to the public; and, although the theoretical opinions on which that explanation

* Siris, par. 143. et seq.—2d Edition.

† Micrographia, p. 103.—An. 1665.

‡ Tractat. Quinque, p. 98. et seq.

was partly founded, no longer exist, yet it should never be forgotten that his experiments and discoveries first pointed out the true path of investigation; and have contributed, in a pre-eminent degree, to advance our knowledge of this most important function. The cause of the unfitness of air, beyond a certain extent, to support life and flame, he proved to arise from the destruction of its pure part, or what has since been called its oxygen gas; and he concluded, that, by the several processes of respiration, combustion, and calcination, it underwent precisely the same changes*. M. Lavoisier, in the following year, investigated these changes with greater accuracy. He confirmed the fact of the disappearance of oxygen gas during respiration; and added, that the residual air of this process differed from that left after the calcination of metals in containing a quantity of carbonic acid†; thus verifying also the fact discovered more than twenty years before by Dr Black.

118. Whence then is this carbonic acid, which is formed in respiration, derived? In the several processes of germination, of vegetation, and of respiration in the inferior classes of animals, it has been shewn, that, as carbonic acid is formed, the oxygenous portion of the air gradually and completely disappears; and that the acid produced bears always a constant proportion to the oxygen gas lost. We have now to shew, that

* Philosophical Transactions, 1776.

† Mem. Acad. 1777.

the same things take place in the respiration of the superior animals, and of man. Mr Davy confined a small mouse in a jar, inverted over mercury, and containing fifteen cubic inches of atmospheric air, previously deprived of its carbonic acid by long exposure to the water of potassa. In 55 minutes, the animal was taken out apparently dying, and of the quantity of oxygen gas originally present in the jar, 2.6 cubic inches had disappeared, and two of carbonic acid were produced. In another experiment, where a mouse was confined in 15.5 cubic inches of the same air, 2.7 of its oxygen gas, in nearly the same time, had disappeared; and 2.1 of carbonic acid were formed*. Mr Davy himself breathed 141 cubic inches of air one-fourth of a minute, making one inspiration and one expiration, seven or eight different times: and he found, by analysing the respired air, that the quantity of oxygen gas lost in each respiration, was from five to six cubic inches, and that of carbonic acid produced from five to 5.5 cubic inches†. Dr Henderson, on examining 100 parts of the air which he had respired four minutes, found, in one experiment, that of the $\frac{22}{100}$ of oxygen gas originally present, $\frac{8}{100}$ had disappeared, and $\frac{7}{100}$ of carbonic acid were produced: in another, $\frac{10}{100}$ of the oxygen gas were lost, and its place was supplied by $\frac{8}{100}$ of carbonic acid: and when, in a third experiment, the loss of oxygen gas fell between the foregoing numbers, or was $\frac{9}{100}$, so

* Researches, p. 437. 443. † Ibid. p. 431.

likewise did the production of carbonic acid, being in that case $\frac{7.5}{100}$ *. This reciprocal increase and diminution of these two gases could spring only out of some necessary connexion betwixt them: and the variations occurring so regularly, and in such small quantities, are inconsistent with the supposition that the oxygen gas, which forms the carbonic acid of respiration, is derived from any other source than that of the inspired air. If there were any such source, why should not this acid be expired, in a quantity beyond what the residual air of the lungs will supply (104.), when hydrogen gas is breathed; or why should it appear only when air containing oxygen gas is respired, and in proportion always to the disappearance of that gas? Why also should this acid be produced, and life for a time be sustained, when animals are confined in pure oxygen gas: and why should death speedily take place and no acid be formed (beyond what the residual air of the lungs will supply), when they are placed wholly in nitrogen gas? These facts decisively shew, that the oxygen gas, which composes, in part, the carbonic acid formed in respiration, is derived from the inspired air alone: and that the production of this acid bears always a constant proportion to the loss of oxygen gas.

119. But, to constitute this acid, carbon, its other ingredient, must be supplied, which, in the inferior animals, has been shewn (76.) necessarily to proceed from the animal system. In the forego-

* Nicholson's Journal, May 1804.

ing experiments, also, where carbonic acid was formed when the animals were confined in jars of pure atmospheric air inverted over mercury, the animals must have furnished the carbon, since no other substance was present which could afford it. This carbon is likewise given out by the blood of animals (100.) after it is withdrawn from the body, in a state capable of uniting with oxygen gas: and, it is generally admitted, that the animal fluids possess the same power of affording carbon, while retained in the living system; to the union of which substance, with the oxygen gas of the inspired air, the formation of the carbonic acid of respiration has been ascribed.

120. Concerning the manner, however, and the place in which the union of these two substances is brought about in the living body, opinions have greatly varied. By some, the carbon of the venal blood has been supposed to attract the oxygen gas of the air through the coats of the pulmonary cells and vessels, and, by uniting with a portion of it, to form the carbonic acid, which is again returned immediately through the coats of these same vessels and cells. According to others, the oxygen gas enters into the blood-vessels, where it loosely combines with the blood in the capillaries of the lungs, and performs a circulation with it: during this circulation, a part of the oxygen unites with the carbon of the blood, so as to form an oxide of that substance, which, on the return of this fluid to the lungs in a venal state, is, by the acquisition of more oxygen, transformed into carbonic acid, and afterwards expelled through the coats of these same capillary vessels. Others, again, imagine the air to enter in-

to the blood-vessels in its entire state, and to be dissolved and afterwards suffer decomposition during the circulation of that fluid : while these changes are going on, a part of the oxygen combines with the carbon of the blood, and forms carbonic acid, which is liberated through the moist coats of the cells and vessels, when the blood returns to the lungs. In all these hypotheses, it is taken for granted, that the carbonic acid is in some way formed by the union of the carbon of the blood with the oxygen gas of the inspired air ; and the chief difference arises from this union being held by some to take place only in the capillaries of the lungs, and by others, through the whole course of the vascular system. But if, as we contend, no part of the inspired air gains admission into the blood-vessels of the lungs, neither of these opinions can be any longer maintained ; for no oxygen gas can, on such grounds, be held to enter into the blood to unite with its carbon, neither could the acid, which it is supposed to form, be afterwards expelled from that fluid.

121. The objection stated above, applies, however, in part only to the opinion of Mr Abernethy, who considers the carbonic acid of respiration to be derived, not from the oxygenous part of the inspired air, but to be simply exhaled from the pulmonary vessels *. As this opinion rests on the belief that the quantity of oxygen gas that disappears in respiration is not sufficient to account for the bulk of carbonic acid produced, it can no longer be maintained,

* Essays, Surgical, &c. p. 146.

if it be shewn that the volume of that gas which is lost, actually exceeds that of the carbonic acid which is formed. Now, this has been amply done in the experiments (118.) already given : and, indeed, the excess of oxygen lost, was so apparent and so constant, as to lead Lavoisier, and others after him, to conclude, that it was employed to form a part of the water expelled from the lungs, by uniting with hydrogen supposed to reside in the blood, just as water and carbonic acid are formed by the combustion of wax and many other substances. It is, moreover, incumbent on those who hold this opinion concerning the production of carbonic acid, to find out some adequate source from whence the oxygen gas, forming the expired carbonic acid, can be derived ; to explain to us, at the same time, what becomes of the inspired oxygen gas which actually disappears ; and why the acid produced bears always so constant a proportion to the disappearance of that gas.

122. Taking for granted, then, that the carbonic acid expired, is formed by the union of the inspired oxygen gas with carbon furnished by the animal system, and that the gas lost exceeds in volume the quantity of acid produced, we proceed next to investigate the amount of this difference and its cause. From the experiments of Doctors Priestley and Crawford (83.), it appears, that when animals are confined in air inverted over mercury till they die, little or no diminution of the bulk of gas takes place : those of M. Lavoisier, however, furnish a different result. He confined a guinea pig in a jar, containing 248 cubic inches of gas, consisting prin-

cipally of oxygen, which was inverted over mercury : in an hour and a quarter, the animal breathed with much difficulty, and, being removed, the air was examined, and found to be diminished in bulk by eight cubic inches : 40 cubic inches more were attracted by the water of potassa, and consequently consisted of carbonic acid; from which it appears, that the volume of acid produced, was one-sixth less than that of oxygen gas which had disappeared *. In a second experiment, another animal of the same species was confined an hour and a half in a cubic foot, or 1728 cubic inches, of oxygen gas, inverted over mercury : the gas suffered a diminution of 55 cubic inches, and 229.5 cubic inches more were attracted by the water of potassa, leaving a residue of pure oxygen †: hence, therefore, the whole loss which the oxygen gas experienced, amounted to 284.5, while the quantity of acid produced was only 229.5, or was $\frac{1}{5.17}$ less in volume than the oxygen which had disappeared. In these experiments, however, the air breathed was not natural to the lungs, and the confinement of the animal was prolonged until he breathed with difficulty, which, on the principles already stated (92.), would necessarily increase the volume of residual air in the lungs, and render the apparent diminution greater than it ought to be. In the experiments made by Mr Davy on his own respiration, the oxygen gas that disappears every minute, is estimated at 31.6 cubic inches, and the

* Mem. Acad. 1780.

† Ann. de Chimie, t. v. p. 261. et seq.

carbonic acid produced at 26.6,—a difference in volume equal to $\frac{1}{6.32}$ * : and Dr Bostock concludes, that in 24 hours, a man consumes somewhat more than 26 cubic feet of oxygen gas, and produces about 22 cubic feet of carbonic acid †, so that the bulk of the acid falls short of that of the oxygen gas by $\frac{1}{6.5}$ of the whole. It follows from all the foregoing facts, that the volume of carbonic acid, produced by the respiration of animals, is, in all cases, from one-fifth to $\frac{1}{6.5}$ less than that of the oxygen gas which has disappeared during that process.

123. From what cause then does this loss of bulk proceed? It cannot arise from an absorption or attraction of the oxygen gas, and subsequent combination of it with the blood : for both of these circumstances are forbidden by the actual structure of the lungs. But we have seen, that, by the conversion of oxygen gas into carbonic acid, in the process of combustion, a diminution of its bulk (11.), amounting nearly to one-seventh, takes place : and we have traced this diminution, in a less degree, when the same change is produced on oxygen gas by the growth of seeds and of plants, by the respiration of the inferior animals, and also by its being placed in contact with blood. It is true, that the degree of this diminution varies, in these several examples, from $\frac{1}{5.17}$ to $\frac{1}{10}$ of the volume of oxygen

* Researches, p. 434.

† Essay on Respiration, p. 99.

gas employed, but this variation does not destroy our belief in the general fact : and if we consider that these calculations have been formed from experiments where the authors had no such circumstance in their contemplation ; that the correct observation of the fact itself requires an attention to many circumstances of much delicacy and difficulty ; and that the composition of carbonic acid is not yet determined with such rigour as to leave no room for doubt, we must be satisfied with such an approximation towards the truth, as our present knowledge entitles us to make. In conformity, therefore, with these views, we venture to conclude, that oxygen gas, by its conversion into carbonic acid, suffers in respiration, as well as in combustion, a diminution of about one-seventh of its bulk : and since this necessary diminution accounts nearly (122.) for the whole difference in volume between the oxygen gas lost, and the carbonic acid produced in respiration, we may farther conclude, that the whole of this oxygen gas has been employed to form the carbonic acid in question.

124. But, notwithstanding this diminution of volume, which the oxygen gas thus experiences by uniting with carbon, the weight of the compound that is formed, is, at the same time, increased. MM. Lavoisier and Seguin estimated the weight of oxygen gas, consumed by a man in 24 hours, at 15661.66 grains : and that of carbonic acid produced in the same space of time, at 17720.89 grains * :

* Bostock on Respiration, p. 83. 86.

so that the weight of the carbonic acid exceeded that of the oxygen gas by 2059.23 grains. This excess must be attributed to the addition of carbon (119.) derived from the animal system ; and which makes it to constitute $\frac{1}{8.6}$ of the weight of the acid that is formed. In the experiments of Mr Davy (122.), the volume of oxygen gas that disappeared every minute, was 31.6 cubic inches, and that of carbonic acid produced 26.6 inches. But one cubic inch of oxygen gas weighs 0.3474 of a grain, and therefore 31.6 cubic inches will weigh 10.97784 grains : and again, one cubic inch of carbonic acid weighs 0.467 of a grain, and therefore 26.6 inches will weigh 12.4222 grains : so that the weight of the carbonic acid exceeds that of the oxygen gas by 1.4444 grains, or the carbon, as before, constitutes $\frac{1}{8.6}$ of the compound. From these facts, it would seem, that the proportion of carbon, which composes the carbonic acid of respiration, is much less than that which forms the same acid in combustion, which, as we before remarked (13.), was estimated by Guyton at one-fifth. But the whole subject is, at present, surrounded with so many difficulties, that nothing more can be expected than an approximation to the truth ; and we must content ourselves, therefore, with stating generally, that, although the carbonic acid formed in respiration be less in bulk than the oxygen gas which disappears, yet that it much exceeds it in weight ; and that this excess is derived from the carbon supplied by the animal system.

125. As the conversion of oxygen gas into carbonic acid is at all times going on in the lungs, du-

ring the continuance of the respiratory process, it is next in order to examine how far the necessary diminution of bulk (123.) which attends that conversion, will go to explain the absolute loss which the air, during its respiration, suffers. The amount of this loss has been already stated to have been very variously estimated : and the cause of this variation in experiments made on animals (83.) confined in jars of air, has been explained. It is our present object to inquire into that degree of diminution which takes place in air that has only once passed through the lungs, and where all the circumstances of the experiment resemble those which occur in the ordinary process of respiration. Dr Goodwyn states, that the volume of air, taken into the lungs at a single inspiration, loses $\frac{1}{50}$, or sometimes only $\frac{1}{60}$ of its bulk, when expelled from those organs by the next succeeding expiration *. Dr Menzies, taking the average amount of the loss which the air suffered by 56 successive respirations, observes, that the volumes of air, received into and expelled from the lungs, were nearly the same †. Several attempts were made by Mr Davy to estimate the degree of diminution experienced by the air in a single respiration ; and it has been shewn that his determinations greatly vary (91.) according to the manner in which the experiment was made. In the only instance where all the circumstances were perfectly natural, the 13 cubic inches of air which he inspired, lost 0.3 of a

* Connexion of Life with Respiration, p. 51.

† Essay on Respiration, p. 22.

cubic inch, or $\frac{1}{43}$ part of their bulk *. Dr Bostock, on the other hand, concludes, that the air, by a single respiration, loses only $\frac{1}{80}$ part of its bulk †. Amid such contradictory results, it is not to be expected that a conclusion can be drawn which shall truly express the amount of the diminution in question: and indeed, from a consideration of the powers which govern respiration, and the various circumstances which sensibly affect that process, we cannot but consider the actual loss of bulk which the air suffers by a single respiration, as in its nature extremely difficult, if not impossible, to determine. All, therefore, that we can at present venture to assert, is, that the difference between the volumes of air inspired and expired, in natural respiration, is extremely small: and to this we may add our belief, that the necessary loss of bulk which oxygen gas suffers, by its conversion into carbonic acid, in that process, may be made sufficient to account for it.

126. Several attempts have been made to ascertain the absolute quantity of oxygen gas consumed in a given time by respiration: but many circumstances concur to render this a matter of very difficult determination. Dr Menzies, from his experiments, was led to conclude, that, in respiration, 36 cubic inches of oxygen gas were, every minute, converted into carbonic acid; and that thus 51840 cubic inches of that gas, equal to 17625.6 grains, were

* Researches, p. 433.

† Essay on Respiration, p. 99—210.

daily consumed. MM. Lavoisier and Seguin estimated the mean consumption of oxygen gas, every 24 hours, at 46037.38 cubic inches, or 15661.66 grains : and the same illustrious philosopher, on repeating his experiments in the following year, obtained nearly the same results, an account of which he was employed in drawing up, when cut off by the murderous tyranny of the French government. Upon the estimate of Mr Davy, that 31.6 cubic inches of oxygen gas are consumed every minute, the amount, in 24 hours, will be 45504 inches, a quantity equal to 15471.36 grains. This estimate, says Dr Bostock, coincides nearly with that of Lavoisier, though it was obtained by a different process, and by the use of a different apparatus : we may therefore conclude, he adds, that between 45 and 46,000 cubic inches, or about 15500 grains, 2 lbs. 8 oz. troy, is the average quantity of oxygen consumed in 24 hours by the respiration of an ordinary man *.

127. Notwithstanding, however, the necessity of oxygen gas to the continuance of respiration, and the great quantity of it that is thus daily consumed, many facts tend to prove, that, by the very constitution of that function, a necessary limit is placed to its consumption : and that this limit is determined, not by the purity of the air employed, but by some circumstances inherent in the animal system. It has been already observed (14. 40.), that the growth of vegetables is retarded by a great superabundance of

* Essay on Respiration, p. 84.

oxygen ; and that, although insects will live a considerable time in this gas, yet their breathing becomes oppressive, and they die (53.) long before the whole of it is consumed. There can be little doubt but that the other classes of inferior animals would, under the same circumstances, suffer in the same manner. In the experiment also made by Lavoisier on the guinea pig (122.), the animal is said to have breathed with much difficulty, although not more than one-fifth of the oxygen gas was consumed : but some experiments of the same author, at a later period, seem in opposition to this fact. In comparing together the phenomena of combustion and respiration, he observes, that much more combustible matter is consumed in a given time in vital air, than in that of the atmosphere, but that the same circumstance does not hold in respiration : for whether animals respire oxygen gas in its pure state, or mixed with a proportion, more or less considerable, of nitrogen gas, the quantity of oxygen which they consume is always the same. If a guinea pig, he adds, be kept for several days in oxygen gas, or in a mixture composed of fifteen parts nitrogen and one of oxygen, preserving constantly these proportions, the animal in both cases continues in his natural state : his respiration and circulation do not sensibly appear to be either accelerated or retarded : his temperature remains the same, and he has only, when the proportion of nitrogen gas is too great, a slight disposition to drowsiness *.

* Mem. Acad. 1789.

128. The results of Mr Davy's experience, however, do not correspond with these conclusions of Lavoisier. He introduced a mouse into a jar containing an atmosphere composed of 10.5 cubic inches of oxygen, and three inches of nitrogen gas. In half an hour, the animal appeared to suffer much, and, in about an hour, lay down on his side, as if dying: in an hour and a quarter he was withdrawn from the jar alive, but motionless. The residual air, on being analysed, was found to have lost only 2.1 cubic inches of its oxygen gas, and consequently 8.4 inches of that gas still remained. Another mouse, which was put at the same time into a jar containing 15.5 cubic inches of atmospheric air, was taken out through the mercury alive, but unable to stand, in 50 minutes: and on analysing the residual air, 2.7 cubic inches of its oxygen were consumed. Hence it appears, that the mouse in atmospheric air consumed nearly one-third more of oxygen in 50 minutes, than the other mouse did in an hour and a quarter, when placed in a jar containing so large a portion of oxygen*. The results of these experiments on mice are corroborated by those made by Mr Davy on his own respiration; for he found, that he consumed much less oxygen gas when he respired it pure, than when, for the same length of time, he breathed atmospheric air; and the quantity of carbonic acid formed in the first case, was but little more than half that obtained by the respiration, for the same time, of atmospheric air†. These ex-

* Davy's Researches, p. 443.

† Ibid. p. 442.

periments differ greatly therefore from those of Lavoisier as to the effects produced by the respiration of oxygen on the animal system ; for, while the latter philosopher informs us, that this gas may be respired for many days without inconvenience, Mr Davy has shewn that the animal dies long before the whole of it is consumed. Trusting, therefore, to the accuracy of Mr Davy's experiments, as in all respects supported by analogy, we infer, that an excess of oxygen gas in the air that is breathed, is not suited to the due maintenance of the respiratory function : and, on the other hand, the oppressive symptoms which the respiration of impure air occasions, as well as the results of Lavoisier's experiments (127.), in which nitrogen superabounded, equally instruct us, that a deficiency of this gas is alike unsuited to it. Consequently, we may conclude, that the atmosphere, as it is naturally composed, is best adapted to the economy of the animal system ; but, that this system is, at the same time, so constituted as to be able to bear great variations in the composition of the air without immediate injury to the powers of animal life.

129. When, however, this variation proceeds to a certain extent, the air is no longer capable of supporting vital action ; but different animals, when confined in given volumes of air, possess the power of prolonging this action in very different degrees. Thus we have seen (53. et seq.), that insects, worms, fishes, and the amphibia, live until all the oxygen gas of the air is nearly or entirely consumed ; while birds die in a given quantity of air before they have consumed two-thirds of its oxygen (84.), and a

mouse and guinea pig expire when about three-fourths of this gas have disappeared. Dr Priestley observed, that if a mouse can stand the first shock of being put into impure air, or has been habituated to it by degrees, he will live a considerable time in air in which other mice will instantaneously die *. The following case, related by the late Dr Percival, seems to prove that a similar observation may be extended to the human subject. An unfortunate man descended into a coal-pit 90 feet in depth, soon after which the sides of the pit fell in. No assistance could be rendered him till the afternoon of the seventh day, at which time he was found sensible and spoke to his companions: his hands and feet were extremely cold, and no pulse could be felt at the wrist: he slept when not aroused to take nourishment, and lived a few hours after being taken out of the pit. The compass of the cavity which he had dug, and in which he was laid on his belly when his companions reached him, was three yards in length and two in breadth, and the stratum of coal was two feet thick. No circulation of air could possibly have taken place through this cavity; and the foulness of the vapours which issued from it, prevented any one, for some time, from venturing to his assistance †. Now, the capacity of a cavity, nine feet long, by six wide and two deep, will be equal to 108 cubic feet; from which, if we deduct three feet, as the space occupied by his body, the actual

* Experiments on Air abridged, vol. iii. p. 257.

† Manchester Memoirs, vol. ii. p. 467.

quantity of air which the cavity contained will be equal to 105 cubic feet. If we suppose this air to have been at first similar in composition to that of the atmosphere, containing $\frac{22}{100}$ of oxygen gas, the quantity of that gas may be taken at 23 cubic feet, by which the function of respiration was supported for more than seven days. But, in ordinary respiration, the daily consumption of oxygen (122.) amounts to 26 cubic feet ; so that, in this case, animal life was protracted for seven days on much less than one-seventh of the usual quantity of that gas. When, however, death does happen to animals confined in a given volume of air, it must arise either from the noxious operation of the nitrogen gas that is always present, or from that of the carbonic acid, which is formed ; or it must proceed from the deficiency, or total absence, of oxygen gas. Now, although nitrogen gas do not of itself support life, yet we have no evidence that it exerts any injurious effect on the animal system. In vegetation (5. 29.), and in the respiration of the inferior animals (73.), it has been shewn to be wholly inactive ; and when, in the experiment of Lavoisier (127.), it constituted $\frac{15}{16}$ of the air employed, a degree of drowsiness only seems to have been induced by it. That it is entirely passive, is still farther confirmed by an experiment of Lavoisier, who found that hydrogen gas, mixed in due proportion with oxygen, would serve the purposes of respiration as well as the air of the atmosphere. We have no proof that nitrogen is able to enter the vessels so as to produce any direct operation on the blood,—an effect which is still farther

forbidden by its incapacity of uniting with that fluid. We may therefore conclude, that nitrogen gas, when respired, neither suffers any change itself, nor produces any direct operation on the animal system.

130. The only other gas to which the death of animals, in these circumstances, can be ascribed, is carbonic acid, which, however, when formed by respiration, does not seem destructive to animal life. Dr Goodwyn observes, that when the same air is breathed several times, so as to increase the quantity of carbonic acid, its noxious operation is to be attributed not to the presence of this acid, but to the deficiency or absence of oxygen gas^{*}; and when Spallanzani, by means of an alkaline substance, abstracted this acid as soon as formed by the respiration of birds and quadrupeds, he did not find that they lived longer in a given bulk of air than when it was suffered to remain[†]. In the foregoing case also, related by Dr Percival, the carbonic acid formed by respiration must have been retained in the cavity, and yet no destructive effect seems to have followed from it. Dr Higgins observes likewise, that debility, convulsions, and death, follow the successive diminution of the oxygen gas of the air in respiration, long before the whole of that gas is consumed, although the carbonic acid that is generated be, in the mean time, carefully withdrawn[‡]. Indeed, we might in this, as in former examples, be

^{*} Connexion of Life with Respiration, p. 66.

[†] Memoirs on Respiration, p. 318.

[‡] Higgins's Minutes of a Society, p. 160.

led to suppose, that neither the carbonic acid formed in respiration, nor the nitrogen gas employed in that process, would exert any positively destructive operation on the animal powers, since both of them must, at all times, necessarily be present in the system; and seeing, moreover, that the abstraction of oxygen gas alone is sufficient to account for the fatal effects which ensue, it must be deemed unnecessary to resort to the supposed agency of any subordinate cause.

CHAP. V.

OF THE SOURCE OF THE CARBON IN VEGETABLES AND ANIMALS BY WHICH THE CHANGES IN THE AIR ARE EFFECTED.

131. **I**N the preceding chapters, it has been generally concluded, that the oxygenous portion of the air is converted into carbonic acid by the processes of germination and vegetation, and by the respiration of animals: we must therefore presume that seeds, and plants, and animals, in some way, furnish the carbon whereby the acid in question can be formed. To inquire into the immediate source of this carbon is the object of our present investigation.

132. It appears, from the results of experiments already related (1.), that if seeds, in a dry state, be exposed to atmospheric air, they neither suffer any change themselves, nor produce any on the air in contact with them: but that if they be confined in water,

they gradually suffer decomposition, forming carbonic acid and carburetted hydrogen gases ; and the faculty of germinating is then destroyed. If, farther, they be first steeped in water, and then placed in nitrogen gas (5.), carbonic acid is, in like manner, formed ; and the same acid is likewise produced when they are simply plunged into a tube of mercury without the aid of any elastic fluid at all ; but, in neither of these cases, do they exhibit any sign of germination. Whatever, in these experiments, may be the source from whence the oxygen gas forming the acid is derived, it must be granted that the seed, in every case, furnished the carbon. But this acid is likewise produced when steeped seeds are made to germinate in atmospheric air (6.), or in oxygen gas, where nothing also but the seed is present from which the carbon can be derived : hence therefore, it follows, that the seed, during its germination, as well as under decomposition, must be able to supply carbon.

133. Does then the carbon, which thus contributes to the formation of the acid, issue directly from the seed, and unite with the oxygen gas of the air exterior to its substance ? or does this oxygen gas previously enter into the seed, and, combining with its carbon, again escape from it in the form of carbonic acid ? If the view which we have taken of the structure of seeds, and of the manner in which the air is affected by their growth, be just, this latter supposition cannot be entertained : and, from the facts stated in the foregoing paragraph, it is sufficiently clear, that, in many cases, the entrance of oxygen gas into the seed is by no means necessary

to abstract its carbon; for carbonic acid was formed by seeds where none of that gas previously existed. From the following facts, it will be seen also, that the seed, during its germination, is able to yield carbon, without the supposed agency of oxygen gas to abstract it. M. Huber observed that seeds did not germinate in air, which had been previously injured by the respiration of bees, although a quantity of oxygen gas was added to it sufficient to carry on germination, if mixed with pure nitrogen gas: at first, he attributed this failure to the presence of the carbonic acid which remained in the residual air, but when he carefully removed this acid by first washing the residual air in lime-water, he still found, that although fresh portions of oxygen were added, no germination took place, until after a very considerable quantity of that gas had been supplied. He found precisely the same thing to happen when he employed the residual air that had served for successive germinations, since fresh seeds would not grow in such air until it was mixed with a quantity of oxygen much more than sufficient to carry on germination in pure nitrogen gas. Reflecting on these circumstances, he conceived, that as seeds yield carbon to unite with the oxygen gas of the air during their germination, that substance might also combine with its nitrogenous portion; and when thus saturated with carbon, the nitrogen might not favour germination if mixed only with its usual quantity of oxygen, because the oxygen gas, having a greater affinity for carbon than the nitrogen, would carry it off from the latter to form carbonic acid, and consequently not leave sufficient oxygen to sup-

port germination. This supposition was rendered the more probable, because he found, that when oxygen gas was mixed with nitrogen thus saturated with carbon, carbonic acid was formed, although the two gases had, previous to their mixture, been carefully washed in lime-water *.

134. That the carbon of the seed had really escaped, and combined with the nitrogen gas in the experiments above stated, is farther evinced by what follows. M. Huber prepared an artificial atmosphere in proportions similar to that of common air, but in which hydrogen was substituted for nitrogen gas: and in this atmosphere he caused successive quantities of lettuce-seeds to germinate, till they ceased any longer to grow. He then carefully washed this mixture of gases in lime-water, until it produced not the slightest discolouration; and, when all the carbonic acid was by this means removed, he added to the residue a portion of oxygen, previously deprived of every particle of carbonic acid, and inflamed the mixture in Volta's eudiometer over lime-water. At the moment of inflammation, the lime-water was rendered very turbid, and a great precipitation of carbonate of lime took place. As no carbonic acid existed in the two gases previous to their inflammation, its production must have arisen from that process; and we must therefore admit, that the carbon which had combined with the hydrogen during germination was again separated from it, and had united with the oxygen gas to form the carbonic acid

* Mém. sur la Germination, p. 41. et seq.

in question. If, after being thus prepared, the two gases, instead of being inflamed, were placed together over lime-water, carbonic acid was, in like manner, formed; but the same result, which in one case was effected in an instant, required, in the other, a much longer time to produce. It is farther remarked, that pure hydrogen gas burns with a lively white flame, but that the gas which has been employed as an atmosphere in germination, yields a tinge more or less blue, although it may have been washed in lime-water. From these facts, says Sennebier, it must be concluded, that hydrogen gas, which has been used in germination, becomes charged with carbon, and that this carbon can have been furnished only by the operation of germination or vegetation, which produce upon this gas the same effect. It is farther proved by these facts, he adds, that growing seeds and plants are not the direct sources of the carbonic acid found in the atmosphere in which they have lived; but that this acid is a combination of the carbon which escapes from these bodies with the oxygen gas of the air *. The decomposition of carburetted hydrogen in the foregoing experiments of M. Huber, accords with the phenomena which take place when it is placed in contact with oxymuriatic acid gas: for all the varieties of carburetted hydrogen gases, if mixed in due proportion with this acid, are converted into water, carbonic acid, and carbonic oxide gas; and if, instead of allowing the mixture to remain at rest, the electric spark

* Sur la Germination, p. 86. et seq.

be taken through it, a slight explosion follows, with a production of carbonic acid and water*.

135. To these facts, tending to prove the emission of carbon from seeds, it may perhaps be objected, that this substance exists in so fixed a state, that it cannot pass off in the attenuated form, which, in these cases, is required; but this objection applies equally to every case where carbonic acid is spontaneously formed. Those who suppose the oxygen gas to enter into the seed, and, by the power of chemical affinity, to abstract its carbon, must likewise admit, that this carbon is raised into a gaseous out of a fixed state, or no carbonic acid could be formed. This power of assuming the gaseous form, differs not only according to the circumstances in which the carbon may be placed, but also according to the state of combination in which it exists. Thus, in its pure state of diamond, a very intense heat is required to effect the union of carbon with oxygen gas: in the example of plumbago, a lower heat will suffice; and in that of charcoal, a much lower still. Not only, however, by these high degrees of heat, but at the ordinary temperature of the atmosphere, may this carbon be separated from its combination with various substances. Mr Cruickshank observed, that the gas which arises from the slow decomposition of humid vegetable matter, is similar to that obtained by the decomposition of camphor or æther by heat†: and many examples have

* Murray's System of Chemistry, vol. ii. p. 385.

† Ibid. vol. ii. p. 384.

been given, in which carbonic acid and carburetted hydrogen gases were formed by moistened seeds, at the ordinary temperature of the atmosphere. Nor is even the decomposition of the seed necessary to extricate its carbon ; for M. Vauquelin found, that moistened seeds, which had yielded carbonic acid in hydrogen gas, would afterwards grow when placed in the open air *. From these facts it follows, that the carbon of vegetable bodies may be made to unite with oxygen gas at a low temperature, as well as at a high one ; and that this matter may be given out by seeds antecedent to their decomposition, as well as under that process.

136. It is more difficult to ascertain the form which carbon assumes, and the means by which, in these various examples, it is brought into that form in which it exists at the moment of its union with oxygen gas. In the processes where heat is employed, its particles are perhaps so changed or attenuated by the operation of that subtile fluid, as to be rendered capable of combining with the oxygen gas presented to them ; but no such operation of heat can be going on where the carbon is made to unite with that gas at the ordinary temperature of the atmosphere. Some other means, therefore, must be sought, which shall be sufficient to reduce the carbon to the state required for such a combination. Now, we have seen, that the carbon of the seed does not combine with the oxygen gas of the air, so long as the seed continues in a dry state ; but when

* Phil. Mag. vol. xxv. p. 223.

it is duly moistened, this carbon, in the same temperature, and under the same circumstances, is yielded by the seed, and combines either with oxygen, nitrogen, or hydrogen gas. No other agent but humidity, therefore, being introduced, to which this variation in effect can be ascribed, we are led to attribute it to that cause; and to believe that carbon, as it exists in seeds, may be so acted on or dissolved by the imbibed water, as to be capable of passing off from the seed with its exhaled moisture, which thus becomes the proper vehicle of it. It is well known, that both seeds and plants contain a large portion of carbon, and it is extremely probable that they derive it from a state of solution in water; for M. Giobert found, that plants did not grow well in a mixture of simple earths until he moistened his soil with water from a dunghill. Now it is certain, from the experiments of Hassenfratz, says Dr Thomson, that this water contains carbon; for when evaporated, it constantly leaves behind a residuum of charcoal. All those manures likewise which act with efficacy and celerity, contain carbon in such a state of combination, that it is soluble in water; and the efficacy of the manure is proportional to the quantity of carbon so soluble*.

137. If then it be granted, that the carbon of decomposing vegetables is largely soluble in water, there seems no reason why the carbon of the seed, which gradually diminishes (20.) during germination, should not be soluble in it also: and since the wa-

* Thomson's Chemistry, vol. iv. p. 253. 256.

ter imbibed is constantly escaping from the seed, there is reason to believe, that it is the proper menstruum and vehicle of this carbon. Hence, in the foregoing experiments of M. Huber (133-4.), the carbon furnished by the seed united first with the oxygen gas, until that gas became saturated, and afterwards combined with the nitrogen or hydrogen, whose affinity for it is not so strong as that of oxygen*. Even after combining with hydrogen, carbon will again leave it to unite with oxygen, either when the two gases are inflamed together by electricity, or are suffered for a few hours to remain at rest. If, however, fresh portions of oxygen be successively supplied, the carbon, afforded by the seed, seems to unite with it alone, and germination regularly proceeds. There does not, in these examples, appear to be any ground for supposing the oxygen gas to be first received into the seed in order to combine with its carbon, and to be afterwards expelled from it in the form of carbonic acid; for no known powers of the seed are capable of performing such an office, and such a supposition is the less necessary, since the seed is able to emit carbon without it. If also, from the mere fact of their combination, the oxygen gas be considered to enter into the seed, and abstract its carbon, the hydrogen gas must be held to do the same; and the same thing may likewise be affirmed of nitrogen gas, with which (133.) the carbon of the seed will combine. Moreover, the seed affords carbon through every

* Murray's System of Chemistry, vol. ii. p. 311.

stage of its decomposition, where we must suppose it to escape, like the other elements of the body, from the dissolution of those affinities by which they were held together. From the whole, therefore, we are led to conclude, that the carbon of the seed, by the action of the imbibed water, is reduced to a state capable of combining with oxygen gas at the ordinary temperature of the atmosphere, and that it passes off from the seed with the moisture exhaled from it.

138. If any cause obstruct the emission of this water and carbon, germination is either partially or wholly prevented, as the following experiments will prove. Equal numbers of steeped peas were supported by two hoops in two equal jars of atmospheric air; but the peas of one jar were previously dipped in oil. Both jars were then inverted over mercury, and placed in a room beside each other. The oiled peas in a few days had sprouted about one-third of an inch, while the radicles of the others were more than an inch in length. The air of the jar in which the oiled peas had been confined was diminished but little by agitation with lime water; while that of the other jar rendered the lime water quite milky, and lost nearly one-fifth of its bulk. The same results were obtained when varnish was applied over the peas instead of oil, very little carbonic acid being formed, and the residual air still retaining $\frac{15}{100}$ of oxygen; while the residual air of the jar in which other peas had for the same time been growing, when freed from carbonic acid, contained no oxygen gas at all. In like manner, if the imbibed water be too rapidly abstracted, the growth of

the peas is checked, and the air in proportion suffers but little change. A jar containing some steeped peas, supported on a hoop, was, as before, inverted over mercury; but within it was previously placed a small glass cup, filled with powdered lime, which floated on the mercury. The jar, during the whole experiment, continued dry; the peas sprouted but little, and shrunk nearly to the same size as when in their dry state; and the lime greatly increased in bulk. The residual air, when examined, suffered no diminution by being shaken with lime water; but it still contained a large portion of oxygen gas; while the air of another jar, placed in similar circumstances, except that it held no lime, had in the same time lost the whole of its oxygenous portion: the peas also were nearly double their original size, and their radicles 1.2 inch long. Hence, therefore, it is clear, that whatever obstructs the emission of moisture and carbon from the seed, or too rapidly draws off the moisture, necessarily puts a stop to those reciprocal changes between the seed and the air, so essential to its future growth.

139. That the carbon which issues from the seed during its germination, is given out in union with the moisture previously imbibed, will, perhaps, appear more evident from what we have next to offer concerning the carbon furnished by plants during their vegetation. It has been shewn (30.), that the presence of oxygen gas is essential to the growth of plants, and that, by the upper surfaces of their leaves (26.), it is converted into carbonic acid: it has been also shewn (39.), that the carbon, which effects this conversion, proceeds neither from the

soil, nor from decayed leaves, but from the living leaf itself. In what manner, then, is the living leaf enabled to exercise this important function? The sap of plants, which is chiefly taken up by the roots, passes through the vessels of the stem to the surface and extremities of the leaf, where it suffers considerable changes (36.), and is then again returned by the vessels of the footstalk to the branches and stem. By this flow of the sap, the function of perspiration in the leaf is supported, and when, from any cause, it diminishes, vegetation seems, in a great degree, to be suspended. This happens to many plants during a cold rain, as well as through the night; and the leaves, in consequence, assume a change of form and condition, which some have supposed to resemble a state of sleep; and in winter, we know, with the cessation of the flow of the sap, vegetation wholly ceases. But the plant, during all these times, is in contact with the atmosphere, and the cessation of the usual vegetative changes cannot, therefore, be attributed to the want of oxygen gas: they must, consequently, proceed from some alteration in the state of the plant itself, which alteration consists in the diminished or suppressed flow of the sap, by which all the vegetable secretions are suspended.

140. If, farther, the leaves of a plant be separated from the stem, they retain life for a considerable time: but the circulation of fluids through them must then of necessity be cut off. Dr Woodhouse exposed handfulls of the leaves of twelve different plants separately to the light of the sun in 40 ounce measures of air for four hours, and its purity was found to be neither increased nor diminished. Af-

ter they had remained sixteen hours in the air, no effect was produced on it. The leaves were fresh gathered, and no decay could be observed in any part of them*. Now, as the only difference in the state of these leaves consisted in their removal from the plant, and consequent cessation of the flow of the sap, by which the usual changes on the air were prevented, it is to that cessation arising from such removal, and not to any other alteration in the structure or condition of the leaves, that this prevention of change is to be ascribed. But this flow of the sap is a proof of the existence of living action in plants, and by its means all the vegetable secretions are carried on: whence it follows, that as the leaves act upon the air, and consequently emit carbon only while this flow continues, its emission is the result thereof, and consequently of a living action also.

141. By smearing over the upper surface of the leaves with varnish or oil, a mechanical obstruction is furnished to the emission of carbon, and the plant dies (26.) just as when the circulation itself is cut off, little or no change, it is believed, being then produced on the oxygen gas of the air. Since then the leaves alone effect changes on the air, and these only while attached to the stem; and since these changes cease, either when the circulation of the sap is suspended or cut off, or a mechanical obstruction is furnished to the escape of the carbon; we must conclude that the carbon, thus emitted by the leaves, is in truth a vegetable excretion, dependent, like

* Nicholson's Journal, July 1802, p. 159.

others, on the due circulation of the fluids of the plant, and varying as that circulation varies. As we observe this carbon, however, only in combination with oxygen gas, its precise form or mode of existence cannot as yet be ascertained; but the fact of its always accompanying the exhalation of moisture from the leaf, leads to the opinion, that it is not only coincident with that excretion, but is carried on likewise by the same structure; the exhaled moisture, as in the case of the seed, serving as the proper vehicle of it. It appears also from the facts already stated (134.), that carbon is afforded by the plant, as well as by the seed, where no oxygen gas was present to unite with it; which shews that its emission depends immediately on the power of the plant, and not on any supposed chemical agency of the air. It may be added, that the experiments of Dr Woodhouse demonstrate likewise, that leaves as well as seeds, under decomposition, form carbonic acid*: but this we notice for the sake of discrimination only, since we are now considering the changes which atmospheric air undergoes from the agency of certain living processes alone.

142. Ascending next, in the order of our inquiry, into the animal kingdom, we find, that, to insects and worms, the presence of oxygen gas is essential to the continuance of living action (53. et seq.), and that they gradually, and, in many instances, completely convert this gas into carbonic acid. No other substance but the animal being present in the

* Nicholson's Journal, June 1802, p. 159.

experiment by which the carbon could be supplied, we must refer its production to that source. The formation of this acid was found also by Spallanzani to depend very much on temperature; for snails consumed more oxygen, and died in equal bulks of air, much sooner when the temperature was mild, than when it was severe *; and, by gradually reducing the temperature, he at length reached that point where the air in which the snails were confined, underwent no change at all; which point he found to be at zero †. The circulation all this time was gradually declining, and at -1° Reaum. the pulsations altogether ceased, the heart remained perfectly at rest, and the whitish fluid usually flowing in the vessels was in a state of stagnation ‡. Nearly similar results were obtained from another species of snails in a hybernating state. In these, the covering or opercle with which they close up their shell in winter, effectually excludes the air on that side, neither does it penetrate their shells elsewhere; for a small glass tube, filled with mercury, being inserted into this covering, and then inverted, sustained a column of 28 inches of that fluid, which corresponded to the height of the mercury in an adjacent barometer. When, however, the covering was punctured so as to admit the air, it instantly forced down the mercury. The small portion of air contained in these shells for several months during the torpid state of the animals, was found in several trials to be of like

* Memoirs on Respiration, p. 150.

† Ibid. p. 152.

‡ Ibid. p. 154.

purity with that of the atmosphere * ; so that when living action had ceased in these animals, no change was produced on the air in contact with them.

143. Nor are these effects peculiar to those animals which change the air by the mediation of their skin : they are common also to those which breathe by lungs. A marmot that was cold to the touch, and so closely rolled up as to be tossed about and irritated in every shape without exhibiting the slightest indication of life, was exposed in a recipient of atmospheric air, inverted over mercury, to a temperature -12° Reaumur. He continued motionless three hours and a half, during which time the mercury in the jar remained stationary : and the air, on being analysed, was found in every respect similar to that of the room in which the experiment was made †. The same animal was then transferred into a jar of carbonic acid at temperature -12.5° Reaumur, where he remained four hours without shewing the least sign of motion, or suffering the smallest injury ; but a rat, put at the same time into the jar, died almost instantly ; and when, in a second experiment, the temperature was raised to zero, the marmot also began to breathe, and then speedily died in a jar of this gas. These facts prove, not only that the carbon, which decomposes the air, is in every case furnished by the animal, but that its emission depends wholly on the state of the circulating fluids ; for, as the circulation increased, declined, or ceased, so likewise did the emission of carbon, and consequent pro-

* Memoirs, p. 197. 203. † Ibid. p. 333. et seq.

duction of carbonic acid : the oxygen gas could not vary the result, being in all cases equally abundant.

144. Does then this emission of carbon proceed from the fluids circulating in the larger vessels of the animal ? Or is it given out from the exhalent structure of the body ? That these fluids may contain carbon as a constituent element, we do not mean to deny : but, that this substance can be emitted through that organized structure which carries on the respiratory function, may, we think, be justly disputed. This structure has been shewn to embrace a large portion of the surface of the bodies of insects, which again is occupied only by the terminations of exhalent vessels, whose office it is to emit fluids, previously separated from the blood, while the blood itself usually circulates in the deeper seated vessels, or even at the centre of the animal. When also the usual changes on the air are suspended, by placing the animal in a cold medium, the carbon might be expected to accumulate in the blood, if that were the source from which it is directly derived, and carbonic acid to be at that time most abundantly produced : but the preceding facts evince, that it is not to the supposed accumulation of this substance in the blood, but to the state and vigour of the circulating fluids, that the production of this acid is to be ascribed. We have therefore no proof that carbon exists in the mass of blood in a state capable of attracting air, or of being attracted by it through the organized structure of the animal : nor even if these attractions were allowed to take place, do the phenomena at all correspond with what, under such a supposition, ought to happen.

145. If, then, the carbon in question be not emitted from the blood while yet in the larger vessels, nor after it has ceased to be in motion, it must be given out by that fluid while in circulation, and after it has entered into the minuter vessels: and thus it becomes an animal excretion, derived, like other excretions, from the blood, and emitted, like them, by some appropriate structure from the surface of the body. Hence any cause, as cold, checking the circulation, restrains the production of this carbon; or, although the circulation be not checked, the emission of this substance is prevented by smearing the bodies of insects (51.) over with unctuous matter, which in consequence causes their death. All that has now been said of insects and snails, applies equally to the marmot and other torpid animals; for the emission of carbon in all, is obedient to the same laws, except that, in the former case, it is given out by the exhalent surface of the body, while, in the latter, it proceeds from the exhalent structure of the lungs. For these reasons, we consider the emission of carbon in these animals to be truly an excretion dependent on the due circulation of their blood, and partaking of all its variations. The experiments of Spallanzani, it may be observed, prove likewise, that under decomposition, animal as well as vegetable substances form carbonic acid in air where no oxygen gas was originally present*,—a fact with which the experiments of Priestley had long before made us acquainted†.

* Memoirs on Respiration, p. 346.

† Priestley on Air, vol. iii. p. 340.

146. To the life of water-snails, muscles, fishes, and the amphibia, a supply of fresh air has been shewn to be equally necessary, its oxygenous portion being, in every instance, more or less completely consumed and changed into carbonic acid ; the carbon for which purpose could be derived only from the animal system. We have not, in this instance, the evidence of experiment to prove that the carbon was emitted by the living power of the animal, that is, only while its circulation continued: nor, although some of these animals are known to suffer torpidity, can we experimentally shew that they then produce no change on the air contained in the water in which they are placed. In every view of the subject, however, analogy is so strongly in our favour, that we cannot hesitate to admit both suppositions to be true: and the termination of the branchial artery in exhalent orifices (65.) on the gills of fishes, would seem to point it out as the structure by which, in that class of animals, the carbon, in combination with the exhaled fluid, escapes. At all events, the blood of a very large number of these animals being devoid of colour, is at least a proof that carbon does not cause the blackness of that fluid ; or, if it still be contended that it does, then blood, which is white, and may be presumed therefore to contain no carbon, can effect the same changes on the air, as that which owes its blackness to this substance.

147. Analogous to the emission of carbonic matter from the surface of the bodies of snails (145.), seems to be that which is carried on by the external and internal surfaces of the human body. The Count de Milly first observed small bubbles of air

to form on the surface of the body when it was immersed in the warm bath : and, by means of a funnel and inverted glass jar, he collected, in a few hours, a considerable quantity of it, which, being examined by M. Lavoisier, was found to precipitate lime-water, and to suffer no diminution from the admixture of nitrous gas *. Mr Cruickshank observed, in repeated trials, that the air of the glass-vessel in which his hand and foot had been some time confined, rendered lime-water turbid, and that a lighted taper burned very dimly in it †. Mr Abernethy exposed his hand for five hours to seven ounce measures of atmospheric air, contained in a jar inverted over mercury : the quantity of air diminished half an ounce, and an ounce more of it rapidly disappeared by agitation with lime-water, and the lime was precipitated : the remaining air being examined by the test of nitrous gas, was found to contain nearly one-sixth less of oxygenous gas, than it did before the experiment. In another similar experiment, continued for nine hours, more than an ounce measure of carbonic acid was produced, and the remaining air contained one-fourth less of oxygen than before ‡. M. Jurine fixed well-dried bottles under his armpits and round his waist, and, after they had remained an hour in these situations, the air in them was much vitiated : and on different occasions was found to be diminished in bulk, and to contain from

* Berlin Transactions for 1777, p. 35.

† On Insensible Perspiration, p. 81. & 82.

‡ Essays, Surgical and Physiological, p. 118.

from four and a half to $\frac{7\frac{1}{2}}{100}$ of carbonic acid. The air confined under the bed-clothes where different persons had slept, he found likewise to contain from four to $\frac{8}{100}$ of carbonic acid. In the stomach also, carbonic acid is formed: and from that organ the proportion of oxygen gas diminishes progressively downwards, the carbonic acid varies in quantity, and the nitrogen gas is uniformly increased*. These experiments prove, that carbonic acid is produced wheresoever the surfaces of the living body are brought into contact with atmospheric air.

148. Whence, then, does this carbonic acid proceed? Dr Ingenhousz, finding that small bubbles of air still escaped from the surface of the body, even although he first carefully removed those which naturally adhered to it, was led to consider them as an aërial transpiration from the skin†. But when Dr Priestley employed rain water, from which the air had been completely expelled by boiling, and detached also the adhering bubbles from his arm, he was never able to collect a single bubble of air‡. Dr Pearson also found, that there was no appearance of aërial bubbles on the surface of the cuticle during bathing in warm water that had been previously boiled, so as to expel the air usually mixed with it§. And M. Jurine, after remaining in the bath for hours together, in temperatures varying

* Mém. de la Société de Med. tom. x.

† Exper. sur les Vegetaux, tom. i. p. 152.

‡ Experiments and Observations, vol. v. p. 103.

§ Hunter's Observations on Animal Œconomy, p. 168.

from 70° to 100° , never could obtain a single bubble of air from his skin. These facts correspond precisely with those already adduced concerning the separation of air from water (45. 47.) by vegetable bodies and other fibrous substances. They prove decisively, that the human body, like other solid bodies, has the power of separating the air naturally contained in water : but that when the water contains no air, none is separated by immersing the body in it. It matters not what the substance is, says Mr Hunter, if it be but warmer than the water ; for a piece of iron heated to 150° , and immersed in water at 70° , will cause the water to part with its air ; but if the iron be ten degrees colder than the water, little or no air will be separated *. The small hairs distributed over the body facilitate, in all probability, this separation of air ; for Mr Abernethy observed, that, on introducing his hand and arm into a jar of water at the temperature of 60° , every pore seemed covered with a little spherule of air, which, on agitating the water, was detached, and quickly rose to the top of the vessel ; and when the water was changed at each repetition of the experiment, a much greater quantity of air was collected, but in moderately warm water he scarcely procured any air †. The air separated in experiments of this nature was, according to Dr Priestley, just that mixture of carbonic acid and partially vitiated air which pump-water generally abounds with : and Dr Klapp,

* On the Animal Economy, p. 168.

† Essays, Surgical, &c. p. 115.

who has lately repeated these experiments in America, found, that the gases collected by holding the arm in an inverted glass-vessel filled with water, differed in no considerable degree from those which had been previously obtained from the water. These facts altogether forbid our drawing any conclusion in favour of an aërial transpiration by the skin, from the mere circumstance of air being separated by the body when it is immersed in certain kinds of water.

149. But Mr Abernethy supports the opinion of an aëriform perspiration by the skin on other grounds, maintaining, that carbonic acid is given out as well when the hand is plunged in a jar of mercury, or is immersed in any kind of air, as when it is placed in contact with oxygen gas *. After detaching the adhering air from the surface of his hand, by moving it for ten minutes in mercury, he introduced it within a jar filled with that fluid, the thermometer standing at about 60° . Minute air bubbles rose at first to the top of the fluid, but more slowly afterwards; and in an hour, when the hand was withdrawn, the air collected was equal in bulk to one scruple of water. In sixteen hours, it equalled half an ounce; and of this, two-thirds were attracted by lime water, and the remainder did not diminish on mixture with nitrous gas: whence he concludes, that it consisted of carbonic acid and nitrogen gas, and that these gases were perspired by the skin. The small quantity of air separated in this experiment, during sixteen hours, is itself a proof that this

* Essays, Surgical, &c. p. 121, 122.

supposed aërial perspiration is very inconsiderable ; and the air obtained is to be considered not as the result of one, but of many experiments ; for we are told, that after the first hour the hand was withdrawn ; and there can be little doubt but that this was several times repeated before the experiment was closed. By so frequent an introduction of the hand, it is very probable that small quantities of adhering air would be repeatedly carried in, which is confirmed by the observation of Mr Abernethy, that the separation of the air was greatest on first introducing the hand *. He farther says, that when in temperatures between 60° and 70° , he kept his hand five hours in seven ounce measures of nitrogen gas, confined in a jar over mercury, there appeared no difference in the quantity of the air ; and on passing lime-water into the jar, rather more than one ounce of carbonic acid was produced, when no oxygen was present. The same results, nearly, were obtained, when hydrogen and other gases were employed †.

150. These experiments of Mr Abernethy have lately been repeated in Philadelphia by Dr Klapp, in the presence of Dr Woodhouse, Professor of Chemistry in that university ; and with completely different results. This gentleman carefully separated all the adhering air from his hand and wrist, by moving them in different directions under the surface of the mercury, for ten minutes, as Mr Abernethy had done : they were then introduced into the inverted glass-vessel, filled with the same fluid, at tem-

* Essays, Surgical, &c. p. 112.

† Ibid. p. 121.

perature 73° , and retained for more than three hours. During all this time, not a bubble, either of carbonic acid or nitrogen gas, was seen to emanate from the skin; and the experiment was a second time repeated with precisely the same result. He next immersed his hand and wrist in very pure lime-water, of the temperature of 70° , and held them in that situation, in different experiments, for one and two hours, during which times no carbonate of lime was formed; but, on the contrary, the lime-water, when the hand was withdrawn, was as transparent as before it was introduced. He next placed his hand and wrist, after separating all adhering air from their surface, in four ounce measures of pure hydrogen gas, confined in a vessel inverted over mercury, and kept them in this situation for three hours. The air in the vessel was then examined in the presence of Professor Woodhouse: its volume was not in any degree diminished; neither, on the addition of lime water, could any trace of carbonic acid be discerned in it*. In addition to these experiments, disproving the existence of an aëriiform function in the skin, it may be observed, that if such a function did really exist, the bulk of air ought to be increased, when carbonic acid is formed by placing the arm in a jar of atmospheric air; but, according to Mr Abernethy himself, the air was considerably diminished. All the arguments already urged against the absorption of aëriiform fluids, forbid us to suppose that the oxygen gas which disappears, is taken up

* Inaugural Dissertation, Philadelphia, 1805, p. 21. et seq.

by a natural function of the skin : and the apparently decisive experiments of Dr Klapp, prove, that no such absorption of oxygen is necessary, because no emission of carbonic acid, or of any other aëriform fluid, can take place from that organ. The production of carbonic acid in the stomach, with the diminution of oxygen, and progressive increase of nitrogen gas in the intestines, observed by M. Jurine, appear necessarily to arise from the longer time in which the air remained in contact with the body, and the greater degree of decomposition which it in consequence underwent.

151. If, then, there be no proof that oxygen gas enters into the body through the skin, and if it be also allowed that no carbonic acid can be emitted from that organ, it must be granted, that the acid actually produced (147.), is formed exterior to the skin itself ; and the disappearance of the oxygen gas, in proportion to the production of this acid, leads at once to the opinion that it is formed in part out of that gas. M. Jurine found accordingly, that as much acid was formed out of a given bulk of air, when his arm was confined in it for one hour, as when the experiment was continued for three or four hours, which, it appears to us, could have arisen only from all the oxygen gas having within the first hour been changed. For the formation of this acid, however, carbon, its other ingredient, must be in some way supplied ; and as, in these experiments, no other substance but the skin was in contact with the air, from which it could be derived, we must ascribe the production of the acid to the spontaneous union of carbon, furnished by the skin, with

the oxygen gas of the air ; and this carbon we conceive to be given out by its exhalent function.

152. In vegetables, and in the inferior animals, it has been shewn, that carbonic matter is yielded only while the circulation of their fluids continues : and there are some facts related by M. Jurine which render it very probable, that the same thing holds true concerning the carbon which is furnished by the human skin. He found, that in given times, the greatest quantity of carbonic acid was formed by the skin of a person in the vigour of life, next by a boy of ten years of age, and least of all by a man of 70 ; and that its production depended much, likewise, on the season of the year, and the vigour of the cutaneous organs. All these facts seem necessarily to follow from the opinion that this carbonic matter is truly an excretion, depending altogether on the distribution of the animal fluids, and subject to all their variations. Whether, as in other animals, the air is no longer decomposed by the skin when the circulation has wholly ceased, we have not the means of deciding ; but the results of the foregoing experiments of Jurine, lead us to suppose that such is actually the fact. Even at the period in which Mr Cruickshank wrote, it is probable, that, had the composition of carbonic acid been then determined, he would have formed a similar opinion concerning the source of the carbon : for, in referring to his experiments, he observes, “ that (admitting the common theory of fixed air and phlogiston) something passed off with the vapour of insensible perspiration by the skin, which rendered air fixed, and

heavier than atmospheric air *.” By wearing a fleecy hosiery vest also next the skin for a month at a time in the hottest parts of summer, he collected an oily substance which accumulated on the nap: this substance he exposed to a red heat in a silver spoon, and, when burnt, it left behind a black powder resembling in every thing the powder of charcoal. “This all seems to prove,” he adds, “that phlogiston” (by which he can here mean nothing else than carbon) “is emitted from the pores of the skin †.” From the establishment of this power in the skin, we obtain, besides, this additional and important fact,—that carbonic acid may be formed out of the oxygen gas of the atmosphere, when brought into contact with the human body, without the necessity or even the possibility of believing that the carbon entering into its composition, is derived from the venal blood; for the exhalent vessels of the skin are all of arterial structure, and the fluids which they contain and emit, exhibit nothing of the colour or the properties of venal blood.

153. The changes effected on the air by the eggs of birds, may still farther elucidate this subject. The shell of the egg is formed of carbonate of lime, with a small portion of phosphate, united to an organic tissue. Spallanzani found it to be full of holes, through which pass the extremities of very minute vessels, coming off from the chorion, or membrane with which the shell is internally lined: these vessels terminate by small orifices on the surface of the

* On Insensible Perspiration, p. 82. † Ibid. p. 94.

shell, forming a fine and transparent net-work. A hen's egg was placed in three cubic inches of atmospheric air, inverted over mercury: on examining the air at the end of the fourth day, it was somewhat diminished in bulk, its nitrogenous portion remained unaltered, a large quantity of its oxygen had disappeared, and a portion of carbonic acid was produced. This diminution of the oxygen could not be owing to any supposed attraction of it by the substance of the egg; for the shell, when deprived of its contents, was found to produce a similar effect on the air *. As the carbonic acid in this, as well as in the former instances, was formed when the air and the egg were in contact, and neither of them separately could produce it, it is evident that the air must have furnished the gas, while the egg supplied the carbon. It is known that an exhalation is constantly going on from the egg, by which it is daily becoming lighter; and the more rapidly this exhalation proceeds, the sooner does the egg decay. The egg of a sparrow, weighing 29 grains, was exposed on a table in a room varying from 60° to 66° Fahrenheit. By the third day, it had lost one grain: by the fifth day, two grains: by the tenth, three grains: by the seventeenth, four and a half grains: and by the twenty-sixth day, six grains: when broken, its contents were very putrid. A fresh hen's egg, weighing 13 drachms, and exposed in the same room from June 15. to July the 30th, lost in weight 2 drachms 20 grains: while another egg, equally exposed, but

* Memoirs on Respiration, p. 237. et seq.

smeared over with oil, lost, in the same time, only 45 grains. If also eggs be varnished over, or placed under the exhausted receiver, they do not corrupt: and Bomàre mentions the circumstance of three eggs being found in the wall of a church, which, by being protected from the air, were quite fresh after the lapse of 300 years *. These facts prove, that a reciprocal action is going on at all times between the carbon of the egg and the oxygen gas of the air, by which carbonic acid is produced; but which action may, at any time, be interrupted, either by closing the pores of the shell, or by abstracting the pure part of the air. The loss of weight which eggs suffer, arises, in all probability, from the escape of their more watery parts, which pass off through the processes of the chorion described above; and, as the same causes which obstruct the exhalation of moisture, seem also to check the emission of carbon, it is reasonable to suppose, that they both proceed from the same individual structure.

154. We have, in the next and last place, to inquire whence issues the carbon which unites with the oxygen gas of the air to form carbonic acid, in the respiration of man and other warm blooded animals. For the most part, this carbon has been supposed to be derived from the venal blood, either in the course of its transmission through the pulmonary vessels of the lungs, or during its circulation through the capillaries of the system: and to the

* Encyclop. Brit. art. Physiology.

presence of this substance in the blood, the black colour of that fluid has been attributed. Formerly, indeed, this blackness of colour was ascribed to the agency of phlogiston, or of hydrogen: but as the existence of the former principle is now altogether denied, and the latter substance can be shewn to reside in the blood only as a constituent part, as it does in the other fluids and solids which are of various colours, we must reject the opinion of its sufficiency to account for the black colour of the blood. Because also red blood became black when placed in nitrogen gas, by some, that gas has been held to be the cause of its blackness; but, according to Mr Davy, no affinity subsists between nitrogen gas and blood*. The same effect takes place also, when blood is exposed to the contact of other gases, and even *in vacuo* without the agency of any gas at all. Dismissing then these opinions concerning the cause of the black colour of the blood, let us next inquire how far it may, with justice, be attributed to the presence of carbon.

155. That carbon is a constituent element of animal substances, is generally allowed; and that it exists in the blood, and is yielded by it to form carbonic acid, have likewise been admitted: but that, to this carbon, the black colour of the blood is, or can be, owing, has not hitherto been distinctly proved. It remains to be shewn in what state this carbon exists, and by what agency it can produce this colour; for in its pure state of diamond, nothing is farther

* Researches, p. 375.

removed from blackness than carbon. The disappearance of the black colour of the blood, as carbonic acid is formed in respiration, is no proof of such blood losing its colour, in consequence of yielding carbon; for we have seen, that, by living vegetables, by animals, and by the human skin itself, this acid is equally formed where no black blood is present to supply carbon. Dr Lower also relates the case of a young woman bled in the foot, whose blood was as white as milk, and yet she was in good health: and, in two other cases, the blood drawn from a vein in the arm, was of the same complexion*. The loss of the black colour of the blood too may be owing to the acquisition, rather than to the escape, of some colouring matter; or it may arise from some change in the properties of the blood, which shall vary its power of reflecting the rays of light. Neither because carbonic acid is formed by exposing black blood to the air, can we admit this as a proof, that its blackness arose from the presence of carbon; for Dr Girtanner found, that this acid was formed equally by arterial blood (101.), which is red: and we have seen it to be produced by the serum (97.), as well as by the crassamentum of the blood. Other agents also, which are totally independent of carbon, are capable of producing this black colour in the blood. Every one knows, that a part exposed to great cold, speedily becomes black: and Dr Crawford found, that, on immersing a dog, whose temperature was 100° , in water at 45° , the

* Lowthorpe's Abrid. Phil. Trans. vol. iii. p. 239.

blood drawn from his jugular vein, was the darkest he had ever seen *. Lastly, we have endeavoured to prove, that no gases either exist in the blood, or can be transmitted through the cellular and vascular structure interposed between the air and that fluid in the lungs ; consequently, no oxygen gas can enter into the blood to unite with its supposed carbon, nor, if such an union did take place, could the carbonic acid be afterwards expelled from that fluid.

156. If, then, the carbon, which unites with the oxygen gas of the inspired air during respiration, come not directly from the blood flowing through the pulmonary vessels, it must in some other way be supplied by the animal system. It is generally admitted, that the cellular surface of the lungs is furnished with exhalent vessels, like every other surface of the body : and indeed, the excessive quantity of fluid which is sometimes poured out by these vessels into the bronchial cells, is not unfrequently the cause of severe disease. This exhalation of fluid from the lungs, contrary to what is the case in other exhalent surfaces, seems to be chiefly supplied by the watery parts of the venal blood ; for the pulmonary artery in the lungs, not only terminates into veins, but, like other arteries, and like the branchial artery in fishes (65.), has an exhalent termination also. Dr Thruston injected a coloured liquor into the pulmonary artery of a sheep, and the fluid partly passed into the veins, and in part also escaped by the trachea † :

* On Animal Heat, p. 310.

† De usu Respirationis, p. 48. an. 1671.

and Dr Hales having filled the lungs with air, after pouring water into the pulmonary artery of a calf, observed the water to flow so freely into the bronchial cells, as to run out at the windpipe ; but the vessels by which it escaped, were not large enough to permit the colouring particles of the blood to pass *. In like manner, injections will pass from the pulmonary veins into the bronchia ; so that the exhalent function in the lungs is carried on by the whole system of pulmonary vessels. And, indeed, if we reflect how much larger a portion of water the venal blood (from the termination of the trunk of the lymphatic system into the left subclavian vein at only a short distance from the heart) must possess in the lungs, than after it has passed those organs and circulated through the whole body, we cannot but admire this deviation from the ordinary laws of the exhalent system, directed, as it is, to continue and preserve, in sufficient abundance, an excretion apparently so essential to the existence of living action.

157. These exhalent vessels of the lungs, like those of the skin and intestines, appear to be endued with a power, not only of exhaling water, but likewise of emitting carbon ; for water and carbonic acid are expelled from the lungs in respiration, in the same manner as they are produced by the skin (147.) when in contact with atmospheric air. As, therefore, the products of respiration and perspira-

* Statical Essays, vol. ii. p. 72.

tion are in kind precisely similar, we are justified in ascribing their formation to similar laws: and, since it seems to have been demonstrated by direct experiment (150.), that no transpiration of aëriform fluids takes place through the skin, we may presume that none is able to be carried on through the cells and blood-vessels of the lungs. Not only is the cellular surface of the lungs furnished with absorbent and exhalent vessels like that of the skin, but it is supplied from within by the same blood, and exposed from without to the same atmospheric air. It has been shewn also, that the colourless fluids of various animals, are able to effect the same change on the air, as that which is produced by the blood: and that the serum of the blood itself, (which is especially destined to supply the exhalent function), produces on the air the same identical change as it experiences in the lungs: all which circumstances strongly incline us to suppose, that the function of the lungs resembles in kind that of the skin. The proofs likewise already adduced, that the carbon furnished by vegetables (140.), and by the inferior animals, as well those which respire by the skin (142.), as those which breathe by lungs (143.), depends wholly on the due circulation of their fluids, and is, consequently, the result of a living action, are strong presumptive evidence, that the same law obtains in the superior animals, and in man: and seem to authorise the conclusion, that the carbon supplied in human respiration, is truly an animal excretion, carried on by the exhalent vessels of the lungs; and therefore, that it primarily depends, like other excretions, on

the due circulation and distribution of the blood, and is more or less affected by all its variations *.

158. In the inferior animals we have seen (142.3.) that the production of carbonic acid, and consequently of carbon, is very much influenced by external temperature, as this more or less affects the circulation of the blood: and even in the human subject, it has been shewn (152.), that the same causes possess a decided

* Long since this opinion concerning the source of the carbon furnished in animal respiration, was entertained by the author, his friend Dr Nugent has pointed out to him a passage in a French writer, wherein a similar notion is distinctly stated. The author, M. Caron, in opposing the opinion of Dr Goodwyn concerning the entrance of oxygen gas into the blood-vessels, and the emission of carbonic acid from them, has these words:—" Je vais demander ici à nos chymistes modernes, si, pour qu'il se forme de l'air fixé dans la respiration, il ne suffit pas que l'air vital puisse se charger, se souler de l'humeur qui sort des vaisseaux exhalans des poumons: pour moi, je crois que cet effet peut s'opérer tout bonnement de cette manière, sans que l'air vital communique en quelque chose avec le sang: Je suis d'autant plus fondé à le croire, que Goodwyn, ainsi que tous les chymistes modernes, avouent qu'ils ne connoissent pas encore la route que l'oxigène peut prendre pour y parvenir, ni par quelle vertu il peut agir sur lui." In a subsequent paragraph also, he asks, whether, instead of saying with Goodwyn, that a certain quantity of oxygen gas is separated from the inspired air in the lungs by respiration, and a certain quantity of carbonic acid substituted in its place, it would not be better to say:—" Une certaine quantité de gaz oxigène constituant l'air atmosphérique, se charge dans les poumons d'une certaine quantité de l'humeur de la respiration, qui le métamorphose, et en change tellement la nature, que l'oxigène n'est plus reconnoissable, et qu'il est devenu d'acide carbonique *."

* Recherches Critiques, par J. C. F. Caron, p. 29. 51. an. 1798.

influence over the formation of this acid by the skin. M. Jurine not only proved the greater consumption of oxygen gas by the skin during a vigorous action of the cutaneous organs, but likewise shewed, that increase of temperature, muscular exertion, and digestion, had similar effects on the products of respiration. These experiments were followed out with greater accuracy by MM. Lavoisier and Seguin, who found, that a man at rest, with an empty stomach, and in a temperature of 82° , consumed in an hour 1210 cubic inches of oxygen gas ; but that during digestion, the consumption amounted to 18 or 1900 inches. With an empty stomach, and during violent exercise, 3200 inches were consumed in an hour ; and when the same exertion was used after taking food, the quantity was increased to 4600 cubic inches *. The difference in the results of these experiments, is so very great, as to lead to a suspicion, that some considerable error exists ; but, at all events, they seem to prove, that whatever, in a certain degree, accelerates the circulation of the blood, increases the consumption of oxygen gas in the lungs, and as this gas, in natural respiration, disappears only in consequence of its union with carbon, it follows, that the carbonic matter must be proportionally increased, which is a necessary consequence of its being considered an animal excretion, immediately dependent on the motion of the blood.

159. But, granting unto the several classes of vegetables and animals which have passed under our re-

* Mém. de l'Acad. de Scien. 1789, 1790.

view, and to the surfaces of the human lungs and skin, the power of emitting carbon, it follows, since this emission is carried on through the whole period of living action, and is essential to the continuance of it, that some ulterior source must be provided, from whence its supply may be duly maintained. In seeds, this carbon forms a considerable part of their substance, and, during the whole period of germination, its proportion is constantly diminishing (20.), by uniting with the oxygen gas of the air. Before this supply is exhausted, roots are sent off from the seed into the earth, by which it draws in nutritious matter, a large portion of which is considered to be carbon ; and this carbonic matter undergoing the ordinary changes in the assimilating organs, is, in part, like other excreted substances, thrown off by that expansion of the seed which now forms the plant, and flourishes in the open atmosphere. So long as water and heat are duly supplied, so long does living action continue, of which the absorption of carbonic matter by the roots of plants, and its expulsion by the leaves, are natural and necessary consequences ; but the various intermediate changes which it is made to undergo, we do not pretend to explain : they belong, in fact, to another branch of inquiry, and cannot be understood until the functions of nutrition and secretion be more fully ascertained.

160. In the inferior animals, carbon is likewise a necessary constituent substance ; and certain of the laws by which it is expelled from them, we have attempted to ascertain. By them too, the means of acquiring it, must be as constant as its expulsion, during living action, has been shewn to be ; and

from no other source than through the organs of digestion and secretion can it be conceived to be derived. But our knowledge of the theory of these functions in animals, as in vegetables, is extremely limited and imperfect: and while it so continues, no rational explanation of this matter can be expected or obtained.

161. Nearly the same remarks may be made concerning the primary and original source of carbon in the superior animals. To the organs of digestion, assimilation, and secretion alone, are we enabled to trace it; but the mode in which it is reduced to that state in which it is afterwards expelled by the surfaces of the lungs and skin, involves a knowledge of the nature and qualities of our food, of the various and successive changes which it is made to undergo in the system, and of its distribution by the blood to the different organs of secretion, according to the several uses which it is afterwards destined to answer: concerning all of which subjects, we have of late succeeded in getting rid of much error and absurdity, but have not, in any instance, attained to complete knowledge.

CHAP. VI

OF THE PHENOMENA WHICH ARISE FROM THE CHANGES INDUCED ON THE AIR BY THE LIVING FUNCTIONS OF VEGE- TABLES AND ANIMALS.

162. **H**AVING, in the foregoing chapters, endeavoured to trace the immediate source of the carbon furnished by vegetables and animals in the several processes above described, and the changes which it effects in the air that surrounds them, we are prepared next to inquire into the phenomena which arise from, and always accompany these changes. Our conception of these phenomena will be much aided by premising a few fundamental facts regarding the doctrine of heat with which modern chemistry has made us acquainted.

163. The sensation of heat which we experience from certain bodies, is known to depend on the operation of a power to which chemists have applied

the term caloric ; and the same power, when it enters into bodies, not only raises their temperature, but almost universally expands them : hence the sensation of heat, temperature, and expansion, are considered as effects, of which caloric is the immediate cause *. The quantities of caloric contained in homogeneous bodies are proportioned to their temperatures and quantities of matter ; but, in other cases, it is established as a general law, that different bodies in equal quantities, whether estimated by weight or volume, contain unequal quantities of caloric †. This property or power in bodies to contain very different quantities of caloric, has, by some, been termed the *capacity* of bodies for heat, while others express the fact more simply, by employing the phrase *specific caloric* to denote the relative quantities of heat which different bodies contain.

164. The greater the quantity of caloric that enters into bodies, the more do they become expanded ; and if this expansion proceed to a certain ex-

* This distinction between heat, and the effects which it produces, did not escape the observation of the ancients. Theophrastus, in his book *De Igne*, says Bishop Berkeley, distinguisheth between heat and fire. The first he considers as a principle or cause, not that which appeareth to sense as a passion or accident existing in a subject, and which is in truth the effect of that unseen principle. This invisible fire, he adds, is present in all parts of the earth and firmament, though, perhaps, latent and unobserved, till some accident produceth it into act, and renders it visible in its effects *.

† Murray's System of Chemistry, vol. i. p. 350.

* Siris, par. 157, 176.

tent, solids are converted into fluids, and fluids into vapours and airs. The rarity and specific caloric of bodies, however, though intimately connected, are by no means proportional to each other. Solid bodies contain less caloric than fluids, and fluids less than airs; and, when a change of form in these bodies takes place, caloric either enters into, or is given out by them. Dr Black, whose admirable discoveries relating to heat, form so brilliant a period in the history of modern chemistry, found, that a quantity of heat, equal to 140° of Fahrenheit, was required to convert ice into water, although the water still continued at the temperature of 32° : and to raise water into vapour required 800° more, although the temperature of the steam did not exceed that of boiling water. As the caloric employed in producing these changes of form, did not sensibly raise the temperature of bodies, he called it *latent heat*: and he shewed that this latent heat was again given out when the vapour was reduced to water, or the water passed into the state of ice. The permanently elastic fluids are subject to the same laws; for when on being disengaged from a combination, they assume a gaseous form, a large quantity of caloric, in a latent state, unites with them, which is again liberated, whenever, by compression, or by chemical combination, they are reduced to a denser state.

165. Many experiments have been made to discover the comparative quantities of heat, or the specific caloric which, at a given temperature, equal weights of various solid, liquid, and aëriiform bodies contain. Taking the specific caloric of water as 1.0000, Dr Crawford found that of arterial blood

1.0300 ; and that of venal blood 0.8928. Among elastic fluids, the specific heat of oxygen gas, compared with that of water, is as 4.7490 to 1.0000 : of nitrogen, the specific heat is 0.7936 : of atmospheric air, 1.7900 : and of carbonic acid gas, 1.6454 *. This high specific heat of oxygen gas, is productive of very important effects in the operations both of nature and art. The power of this gas to supply heat, says M. Berthollet, is well known, and there is no substance which suffers so much of it to escape by the changes of its constitution. In the formation of water by the combustion of oxygen and hydrogen gases, the greater part of the heat might at first be supposed to proceed from the condensation of the latter ; but solid bodies, as phosphorus, occasion the greatest emission of heat by combining with a given quantity of oxygen ; and when water is decomposed by sulphuric acid and iron, much heat is given out, and nevertheless all the hydrogen of the water resumes its elastic form. This heat therefore must, he adds, have proceeded from a change in the state of the oxygen of the water, which gives only a small part of that which it yields, by combining with the iron, to enable the hydrogen to assume the gaseous form †. We now proceed to apply these facts to the illustration of our present subject.

166. Through the whole of our inquiry it has appeared, that the oxygen gas of the air is converted into carbonic acid by the living processes which have

* Crawford on Animal Heat.

† Chemical Statics, vol. ii. p. 15.

fallen under our examination ; and since, from what has just been stated, the specific caloric of this acid is found to be very little more than one-third of that which the oxygen gas itself previously contained, it necessarily follows, that a large quantity of caloric is liberated whenever this conversion of gases takes place. For this office it appears, from the observations of Berthollet, that oxygen gas is peculiarly fitted, since no other substance gives out so much caloric by the changes of constitution which it suffers : and its powers, in the particular instance of change which we are now considering, may perhaps be still farther aided by the degree of condensation which we have seen it in all cases to undergo. Let us then inquire, whether the presence of caloric be manifest in the several living processes above mentioned, and how far it may be deemed to arise from the changes effected on the oxygenous portion of the air ; and first of all in the instance of germination.

167. Some peas, which had been steeped in water 36 hours, were placed in a small glass-jar standing in a room at temperature 63° . In this situation they remained till signs of germination appeared, when the bulb of a very delicate thermometer was plunged into the midst of them, which had previously been compared with another standing beside it. The standard thermometer continued stationary for more than an hour ; but the one that was plunged among the peas, although at first 0.4° below the former, rose in 20 minutes 0.2° above it, and, in 40 minutes, was 0.4° above it ; but at the end of half an hour more, it had again fallen to 0.2° of superiority only.

In a second trial, after turning the peas out of the jar, and again replacing them with a quantity of fresh air, the difference indicated by the thermometer plunged in the peas, amounted, in half an hour, to one degree, but, at the end of another half hour, it was only 0.5° . The amount of this difference must be expected to vary according to the more or less active manner in which the process is going on; but the result seems to prove, that, during the process of germination, a production of temperature actually takes place. The process of malting affords us an example of the same thing. This is conducted by exciting germination to a certain extent in the seed; and, during the process, a considerable production of temperature takes place; so great, indeed, that in certain circumstances, grain improperly kept has even taken fire. From this fact we may conclude, that during the germination of seeds in the earth, a production of temperature likewise takes place*.

168. From what source then is this increase of temperature derived? It cannot proceed from any kind of action in the seed of a mechanical nature, for its structure is not fitted to produce it: neither can it be derived by communication from an external source, for the surrounding bodies are all colder, and are constantly drawing off the heat. Farther, it occurs only while germination is going on; but during that process, the oxygen gas of the air is constantly changing (8. 9.) into carbonic acid by uniting with carbon (20.) furnished by the seed. In

* Thomson's Chemistry, vol. iv. p. 238.

suffering this change, however, the greater portion of its specific caloric is necessarily yielded (166.) by the oxygen gas, and a rise of temperature in consequence takes place ; and hence, therefore, we must ascribe the production of temperature which occurs in germination, to the decomposition of the oxygenous portion of the air, by the agency of the living seed.

169. As the same changes are produced on the oxygen gas of the air by the growth of plants (30. et seq.), as by the germination of seeds, the same extrication of its heat may reasonably be expected to follow. The sudden thaw of snow lying on grass, while that on the adjoining gravel continues unthawed ; the fact of the moisture of dead sticks freezing, while growing twigs are not at all affected ; and of herbaceous plants resisting often degrees of cold which freeze large bodies of water,—all, says Sir Charles Blagden, seem to shew that vegetables possess a power of generating heat *. Mr Hunter also found the leaves of plants to resist freezing longer than water. A growing fir-shoot, and the leaf of a bean, were laid on a cold mixture of the temperature of 28° , and in some minutes they had thawed the surface on which they lay. This did not arise from the greater warmth of the leaves when first applied, for, on removing the fir-shoot to another part, the same effect was produced †. By applying likewise thermometers to the internal parts of vegetables, he discovered them to possess a temperature above

* Philosophical Transactions, 1775.

† Ibid.

that of the atmosphere, but less than that of cold-blooded animals. When the atmosphere was below 56° , the temperature of the tree was always above it; but when the weather was warm, the heat of the tree was several degrees lower*.

170. But the most remarkable and decisive facts, relating to the temperature of vegetables, are contained in the following account of M. Hubert, concerning the heat given out by the *spadices* of the *arum cordifolium* during the process of fecundation. This plant grows in Madagascar and the Isle of France: its flowers exhale a strong and rather pleasant odour. About sun-rise, M. Hubert tied five spadices, which had unfolded during the night, round the bulb of a thermometer, and the mercury rose to 44° ; while another instrument of comparison, at six o'clock in the evening, was only 19° . At eight next morning, the standard thermometer was only 21° , and that used in the experiment had fallen to 42° ; and by nine at night had sunk to 28° , while the first remained at 21° . The next day, at nine in the morning, the thermometer of experiment followed the ordinary course. These trials were repeated seven or eight times with similar results. When surrounded with very fine spadices, the mercury rose to 45° , but reached only to 42° with the smallest; and, in one instance, where the thermometer was surrounded by twelve flowers, the *maximum* of heat was 49.5° . The male parts of six spadices raised the thermometer to 41° , while the same number of the female parts of the flowers raised it only to 28° .

* Encyclop. Brit. art. Physiology.

or 30°. No variation in the results occurred, whether the experiments were repeated in a dry room, or under the shade of thick and humid trees.

171. M. Hubert next endeavoured to ascertain the part of the spadix to which this increase of temperature was owing. He had found that the *medulla*, or pith, raised the thermometer, when plunged into it, in the same way as the exterior surfaces had done; but reflecting that the heat of the pith might arise only from the exterior surface, he removed this latter from four spadices without touching the medulla. These medullæ were then tied round a thermometer, which, at sun-rise, was at 17°, but no sign of increased temperature occurred during 24 hours, and the uncovered spadices withered towards the middle of the day. At the same time, he tied the removed surfaces of the spadices round the bulb of another thermometer, and it rose to 39°. This he repeated several times, which convinced him, that this singular faculty is possessed by the exterior surfaces of the spadices, and within the thickness of $\frac{1}{12}$ of an inch at most. If the spadices of the plant were divided some time before the development of this heat, the fluid that escaped from the divided portions was colourless, which is not the case when the heat has been previously given out.

172. Lastly, M. Hubert made experiments to discover the circumstances necessary to the production of this great increase of temperature. He found that if the spadices were closely covered with a cloth dipped in olive oil, grease, or tallow, no increase of temperature took place: and that if, at their highest temperature, they were plunged into cold water,

their heat quickly disappeared, but again returned, in 25 or 30 minutes, on their being withdrawn. When they were covered with oil or honey, the production of heat in the spadices was suspended for about an hour; and a spadix covered with starch gave no indication of increased temperature till its covering dried and fell off in small portions. Other species of the same genus were found to possess similar properties of producing heat; and it is concluded, that this property is confined to the outer surface of the spadix, that it is independent of light, but that the contact of air is necessary to its production. It was moreover proved, that the air in which these flowers had grown, had suffered considerable changes: for it extinguished a lighted taper, and a chick was suffocated in a closed jar in which several spadices had remained five hours, but recovered on being withdrawn *. These facts, in connection with those before related, sufficiently prove that vegetables possess, during their growth, a temperature above that of the ambient air, and that the contact of this air is essential to its production; and, since it has been shewn, that the oxygen gas of the air is converted (38.) into carbonic acid, by carbon exhaled (141.) by the living plant, whereby the greater part of the specific caloric (166.) of that gas is disengaged, it is to this change of composition in the air, and consequent extrication of its heat, that the increased temperature observed in plants, during their vegetation, is to be ascribed.

* Voyage dans les Isles des mers d'Afrique, par M. Bory de St Vincent.

173. We proceed next to speak of the heat of those animals, which, from the low temperature they possess, have been denominated cold-blooded, and this we shall do in the same order in which they have been already considered. With regard to *insects*, Dr Martine observes, that the whole tribe is commonly brought under the class of cold animals. Caterpillars have but a small degree of heat, about a division or two, above that of the air they live in; but the heat of a swarm of bees raises a thermometer buried among them, above 97° ,—a degree of heat nothing inferior to our own *. Some insects, as flies and wasps, can sustain a loss of heat without losing life, but a bee cannot †. Coleopterous insects become torpid at 34° : at 36° , they move slowly and with difficulty, and, at a lower temperature, their muscles cease to be irritable ‡. During their state of dormancy, the chrysalides of many insects may be frozen without destroying their power of recovering action §.

174. In the class *vermes*, Spallanzani observes, that when a snail or slug is insulated in a jar of atmospheric air, a thermometer placed in the jar will continue stationary; but when several are confined together, the mercury rises one-tenth, one-seventh, and even one-fifth of a degree, and in oxygen gas one-third of a degree; from which he concludes, that snails and slugs, in decomposing oxygen gas,

* Martine on Thermometers, p. 140, 141.

† Hunter's Observations on Animal Economy, p. 108.

‡ Carlisle Phil. Trans. 1805, p. 25. § Ibid. p. 18.

give out caloric enough to be sensible to the thermometer *. This experiment we repeated, by confining several snails in a pint jar of air, from the top of which a small thermometer was suspended, and at the bottom a glass of lime-water was placed. A film of carbonate of lime soon overspread the lime-water, the inside of the jar was dimmed by moisture, and the mercury in the thermometer rose at the same time nearly one degree. Dr Martine says, that from the result of several trials which he made, snails were about two degrees warmer than the air †. Mr Hunter found the lungs of snails 38° , when the atmosphere was 34° ; and, in other instances, snails were six and seven degrees above the atmosphere, when it was so low as 30° . Earth worms he found 58.5° , when the atmosphere was 56° ; and, in other trials, the worms exceeded by four, leeches by three, and slugs by four degrees the temperature of the ambient air ‡. The temperature of a snail, which was 44° , sank, on exposure to a cold mixture, down to 31° , and then froze; and several leeches froze likewise when reduced to 31° §. In all these experiments, the animals, when thawed, were found to be dead; but Mr Carlisle says, that the garden snail may be frozen, during its state of dormancy, without destroying its muscular irritability ||.

* Memoirs on Respiration, p. 255. 258.

† On Thermometers, p. 141.

‡ Treatise on the Blood, p. 298. et seq.

§ Observations on the Animal Economy, p. 105.

|| Philosophical Transactions, 1805, p. 18.

175. The class of fishes possesses also a temperature above that of the medium they inhabit. In flounders, whittings, cod-fish, and haddocks, the temperature, according to Dr Martine, was scarcely a degree more than that of the water they were swimming in, even when that was so low as 41° . Nor are the red-blooded fishes much warmer than the white ones; for trouts were but 62° , when the river water they were swimming in was 61° ; and carp and eels hardly exceeded the heat of the water they inhabited *. Mr Hunter, however, found the mercury in the thermometer to stand at 69° in the stomach of a carp, when the water in the pond from which he was taken, was only 65.5° . That fishes part with this excess of heat to the colder bodies which surround them, we learn also from the experiments of the same author. He put two carp into a glass-vessel with common river water, and then placed the vessel in a freezing mixture: and, as the water in the vessel did not freeze fast enough, snow was added so as to render the whole thick. The snow round the carp melted, and more being added, it melted also, till at last he grew tired of putting it in, and left them to freeze by the joint operation of the freezing mixture, and the natural cold of the atmosphere. At length they froze, but did not, when thawed, recover life with flexibility †; but, that both snakes and fishes, after being frozen, have still retained so much of life, as

* On Thermometers, p. 141.

† Observations on the Animal Economy, p. 89.

when thawed to recover vital action, is a fact, says Mr Hunter, so well attested, that we are bound to believe it.

176. Amphibious animals exhibit a great variety in the structure of the respiratory organs, and, consequently, in the degrees of animal heat. Frogs and land tortoises possess a temperature about five degrees higher than that of the medium they inhabit, according to Dr Martine. The same may be said of sea tortoises, toads, vipers, and all the serpent kind, all of whom have lungs of the same fabric, and the same cold constitution of body *. Mr Hunter observed, that the frog and toad were about four or five degrees warmer than the atmosphere when it was at 35° or 36° : and that some hours after death, they gradually fell down to the temperature of the surrounding air †. This difference of temperature appears to increase in a warmer atmosphere: for Mr Carlisle kept three frogs for many days in an equable atmosphere of 54° , and their stomachs preserved a temperature of 62° ‡. In an atmosphere of 58° , Mr Hunter found the thermometer, introduced into the stomach of a healthy viper, to stand at 68° ; but, after the animal was put into a pan, and the pan into a cold mixture of 10° , where it remained about ten minutes, the heat was reduced to 37° , and in twenty minutes more to 31° , nor did it sink lower: its tail now began to freeze,

* Essay on Thermometers, p. 142.

† Treatise on the Blood, p. 298.

‡ Philosophical Transactions, 1805.

and the animal was very weak. A frog also, whose temperature was 44° , when put into a cold mixture, soon fell down to 31° ; and beyond this point it was not possible to lessen the heat without destroying the animal *. A toad being placed in cold water just deep enough not to cover his mouth, the whole was put into a cold mixture between 10° and 15° . The water froze around the toad, and, as it were, closed him in, but he did not die, and therefore was not frozen. Why the animals, mentioned in these experiments, died before they were frozen, while those which are exposed to the atmosphere in very cold climates do not die, is a point which Mr Hunter does not pretend to determine; not knowing the difference, he says, between the effects of a natural and artificial cold †.

177. The experiments of Mr Hunter farther prove, that the temperature of most of the foregoing animals not only falls rapidly in a colder medium, but that it rises more quickly in a warmer one than that of those which possess a higher standard temperature. In the stomach of a frog, the thermometer rose from 45° to 49° ; the animal was then placed in an atmosphere made warm by heated water, where it remained for twenty minutes, and, upon introducing the thermometer again into the stomach, the mercury rose to 64° †. A healthy viper was put into an atmosphere of 108° , and, in seven minutes, the heat of the animal, both in the stomach and

* Observations on Animal Œconomy, p. 104.

† Ibid. p. 89, 90.

‡ Ibid. p. 90.

anus, was found to be 92.5° , beyond which it could not be raised in the above heat. An eel, very weak, whose heat was 44° , which was nearly that of the atmosphere, was put into water heated to 65° , for fifteen minutes; and upon examination, it was of the same degree of heat with the water. The heat of a tench was, in ten minutes, raised from 41° to 55° both in the stomach and rectum, by being put into water at 65° . He found also, that a living and a dead tench, and a living and a dead eel, put together into warm water, received heat equally fast: and when they were exposed to cold, both the living and the dead admitted the cold likewise with equal quickness*. Hence, therefore, the animal heat, in all the classes of animals hitherto mentioned, whether they inhabit the air or the water, seems to follow nearly that of the medium in which they are placed; and their standard temperature cannot, in consequence, be restricted to any fixed point, but must be considered always in relation to that of their surrounding medium.

178. Notwithstanding, however, the low degree of heat which these several classes of animals possess, hardly, in some instances, exceeding that of the medium in which they live, yet this small excess is a proof that they possess within themselves a power of producing heat. The loss of heat which insects (173.) suffer under cold, the fall of temperature which many of the vermes class (174.) undergo from the same cause, the melting of snow (175.) by

* Observations on Animal Economy, p. 104, 105.

the heat of fishes, and the decline of animal heat which the amphibia (176.), when exposed to great cold, experience, all demonstrate, that the surrounding medium, whether it be air or water, is constantly drawing off their heat, which renders necessary as constant a reproduction of it.

179. By what process, then, or from what source, is this superiority of temperature derived? It does not, says Mr Hunter, depend on the motion of the blood, as some have supposed, because it likewise belongs to animals which have no circulation: neither can it be said to depend on the nervous system, for it is found in animals which have neither brain nor nerves. It is probable, he adds, that it arises from some principle, so connected with life, that it can and does act independently of circulation, sensation, and volition; and is that power which preserves and regulates the internal machine*. This supposition in no degree removes the difficulty. Of the principle of life, or that power which enables organized bodies to exhibit living action, we can form some idea, although we know not its nature, just as the astronomer speaks of gravitation without pretending to define what it is; but of another principle connected with life, and producing sensible effects in the system, independently of all the animal functions, we certainly know nothing; and, fortunately for science, the admission of such imaginary principles, or agents, is now banished from all chaste philosophy. Admitting also, with Mr Hunter, the insuffi-

* Observations on the Animal Economy, p. 91.

ciency of the motion of the blood to account for animal heat, it does not follow that the temperature of animals is wholly independent of it. Dr Jenner, whose discovery of vaccination has conferred so much honour on himself, and such incalculable benefits on mankind, at the request of Mr Hunter, examined the temperature of a hedgehog in its torpid state. In an atmosphere of 44° , its temperature was in the pelvis but 45° , and at the diaphragm 48.5° : and when the atmosphere was reduced to 26° , the heat of the hedgehog in the abdomen did not exceed 30° ; but in summer, when the air is at 78° , and the animal in an active state, its temperature rises to 95° in the pelvis, and to 97° at the diaphragm *. It is sufficiently evident from these facts, that, during a state of torpor, the temperature, even of the warm-blooded animals, exceeds only in a small degree that of the atmosphere by which their torpor is induced.

180. But on passing into this torpid state, under which the temperature so greatly falls, the motion of the blood in animals gradually declines (142. 3.), and at length, in some cases, wholly ceases. All the secretory functions of the animal must, at this period, be suspended, in consequence of which the air in contact with it (142. 3.) undergoes no change; but when heat is restored, the blood again renews its motion, the secretory functions return, and the air undergoes its accustomed changes. These changes consist in the conversion of its oxygen gas (53. et seq.) into carbonic acid, by carbon emitted (145.) by

* Observations on the Animal Economy, p. 99.

these several classes of animals through the medium of their respiratory organs. By these means, a quantity of the specific caloric of that gas (166.) is at the instant set free : and to this constant liberation of caloric by the perpetual decomposition of the air, do we ascribe that superiority of temperature above the surrounding medium which these animals, as well as vegetables (168. 172.) during the continuance of living action, are enabled to exhibit and preserve.

181. We now pass on to the consideration of animal heat in the warm-blooded animals, including the mammalia and birds. In man, the ordinary standard temperature near the surface of the body reaches to about 98° ; in most of the *mammalia*, it somewhat exceeds that of man, and in birds it rises still higher than in the mammalia. Dogs, cats, sheep, oxen, and swine, raise the thermometer four or five divisions higher than man, viz. to 100° , 101° , 102° , and some to 103° , or a little more *. The heat of a puppy, at the diaphragm, was found by Mr Hunter to be 102° : that of a mouse 99° : in the right side of the heart of a dog, the temperature was 101° : in the rectum of an ox 99.5° : and the same in the rabbit †. Mr Carlisle found the temperature of the flowing blood in the horse to be 103° : in pigs, it was 106° and 107° : in the ox, 103° : in sheep, 105.5° , which temperatures, as he observes, are considerably higher than the common estimation ‡. In

* Martine on Thermometers, p. 145.

† Observations on the Animal Economy, p. 100. et seq.

‡ Philosophical Transactions, 1805, p. 22.

those quadrupeds also which mostly inhabit the waters, the temperature is quite as high. Dr Martine found the heat of the skin of a sea-calf to be near 102° , and, in the cavity of the abdomen, it was about a division higher *. Such, too, is the case in the cetaceous order of animals, the temperature of the whale and porpoise being as high as that of land animals. In all these animals, the organs of respiration and circulation, in their general structure and actions, resemble nearly those of man, with such difference only as the peculiar configuration and modes of life of the animal necessarily introduce.

182. But in birds, which have been stated to possess the highest temperature of all animals, the lungs are differently constituted, and are much larger in proportion to the animal than in other cases. Instead of lying loosely in the chest, and yielding to its alternate contractions and dilatations as in the mammalia class, they adhere to the thin transparent membrane which covers their lower surface, and performs the office of a diaphragm: besides this attachment, they are also connected to the ribs and sides of the vertebræ. In the larger cavities of the body, and in the interstices about the breast and axilla, are placed air-bags of different sizes, some of which communicate immediately with each other, and all may be said to have a communication by means of the lungs. The bones also, which, in other animals, are filled with medullary matter, are, in birds, receptacles for air. Some of these, as the sternum, ribs, and verte-

* On Thermometers, p. 146.

bræ, have their internal substance divided into innumerable cells ; while others, as the shoulder and thigh bones, are hollowed out into one large canal, with sometimes a few bony columns running across at the extremities ; and, at that end of the bones next to the trunk of the bird, several holes or openings are placed. There are openings in the lungs, by which the air they receive is transmitted to other parts ; and the diaphragm also is perforated with holes of a considerable size, to each of which is joined a distinct membranous bag, which, being continued through the whole of the abdomen, is retained in its proper situation, by being attached to the back or sides of that cavity. Each bag receives air from the lungs through its respective opening, and such bags extend over the whole abdomen. At their superior part, the lungs communicate with the large cells of a loose net-work, situated on the anterior part of the breast ; and when these are distended with air, the size of the part is considerably increased, as is plainly seen in the turkey-cock, the pouting pigeon, and in the breast of the goose when she cackles. These cells in the breast communicate with others in the axilla, which again communicate with the os humeri, by small openings near the head of that bone. The posterior edges of the lungs open into the cells of the bodies of the vertebræ, into those of the ribs, the canal of the spinal marrow, and into the cells of the bones of the pelvis, from which parts the air finds a passage into the cavity of the thigh bone. Thus, the cells, situated in the soft parts and in the bones of birds, can be furnished with air through the medium of the lungs.

183. Mr Hunter, from whom the foregoing description of the respiratory organs of birds is taken, made several experiments upon the breathing of these animals. He found, by making an opening into the belly of a cock, and introducing a silver canula, previously to tying up the trachea, that the animal breathed by this opening, and might have lived, but for an inflammation of the bowels supervening, which, by adhesion, cut off the communication with the air. He next cut the wing through the os humeri, and, tying up the trachea, found that the air passed to and from the lungs by the canal in this bone. The same experiment was made with the os femoris of a young hawk, and was attended with nearly the like success: but the difficulty of breathing was greater than in the former case, and the animal soon died*. From the great size and peculiar communications of the respiratory organs of birds, it may be presumed, that, in proportion to their bulk, they respire a very large quantity of air; and accordingly, it has been found by experiment, that, in a given time, they consume more oxygen than other animals of the same size, and therefore die sooner in a given volume of air. With regard to their temperature, Dr Martine found them to be warmer than quadrupeds by three or four degrees: for the thermometer being lodged in the groin of ducks, geese, hens, pigeons, partridges, swallows, &c. the mercury was raised to 103° , 104° , 105° , 106° , and 107° : and, in

* Observations on the Animal Economy, p. 81.

a hen hatching eggs to 108° *. Mr Hunter also found the heat of the hen to raise the mercury from 103° to 104° , when introduced within the rectum †.

184. But the degree of heat in animals varies, not only at different times under a change of circumstances, but also in different parts of the same animal, where all the circumstances continue the same. Some examples of this fact have already been incidentally mentioned as occurring in different animals. Under the tongue of a man, Mr Hunter found the degree of heat to be 97° : at one inch within the urethra, 92° : at two inches 93° : at four inches 94° : and at the bulb 97° . The heat of the rectum in the same man was 98.5° . In the mouse, when the atmosphere was 60° , the mercury in the thermometer stood in the pelvis at 96.8° , and at the diaphragm 99° . In the rectum of a dog, the temperature was 100.5° : in the liver, 100.7° : and in the right ventricle of the heart and stomach, it was exactly 101° ‡. The temperature of a horse, killed by dividing the spinal marrow and large blood-vessels, was, in the colon, 98° : in the stomach 101° : and in the spleen 103° , according to Mr Carlisle, when the atmosphere was at 30° . The urine of the same animal was 97° , and the flowing blood 103° . The water flowing from a tapped person was 101° , but, at the surface, the temperature was 96° , the atmosphere at the time being 43° §. From these facts, it is plain, that eve-

* On Thermometers, p. 147.

† Observations on the Animal Economy, p. 103.

‡ Ibid. p. 94. et seq.

§ Philosophical Transactions, 1805, p. 22.

ry part of an animal is not of the same degree of heat : and that the more interior and vital parts possess the highest temperature.

185. We have seen that animals which ordinarily possess a degree of heat but little above that of their surrounding medium, very readily adapt their temperature (177.) to all the variations of that medium. In man, however, and other warm-blooded animals, the system is able to bear very great changes of temperature without a corresponding change in the degree of animal heat. Our sensations, indeed, often apprise us of very slight alterations in the temperature of surrounding bodies, when no perceptible difference, ascertainable by the thermometer, exists. These sensations again, are not only influenced by the general healthy condition of the body, but by the habits with regard to heat and cold which we have been accustomed to indulge. Thus persons who clothe themselves warmly, or live generally in uniform temperatures, are affected by slight variations, which others of hardier habits totally disregard : and those parts of the body which are commonly exposed to the irregularities of the season, are less susceptible of the sensations of heat and cold than such as are more protected from them. The sensation also of heat or of cold, when present in any part of the body, is not only in a great degree independent of the actual temperature of that part, but depends immediately on the previous state or condition of the part itself with regard to sensation ; for the same substance will feel hot or cold when applied to a part, according as that part previously possessed the sensation of heat or cold relatively to

that substance. If, for example, the hands be greatly cooled, and then plunged into fresh pump-water, the water will communicate the sensation of heat, while the same water, at another time, when the hands were previously warmed, would impress the sensation of cold. In like manner, we are often not conscious of any feeling of cold in our hands or feet, but if we apply them to a less exposed and warmer part of the body, the feeling of cold is impressed on that part. The same fallacy with regard to the actual temperature of the body, as indicated by our sensations, exists, when those sensations are connected with internal causes; for a general feeling of cold not unfrequently extends over the body, where no variation in its actual temperature, or in that of the surrounding bodies, has occurred. “In the beginning of an ague fit, when I was all shivering and under a great sense of cold,” says Dr Martine, “my skin was two or three degrees warmer than in a natural healthy state*.”

186. But the actual temperature of the human body, is not only in a great degree independent of sensation, but also of the variations in temperature which the surrounding medium suffers. Governor Ellis, in the year 1758, observed, that a person remaining in a medium higher in temperature than that of his own body, preserved, nevertheless, his natural standard heat. Dr Fordyce went successively into three rooms heated to 90°, 110°, and 120°. He staid in the first room five minutes, which gently

* On Thermometers, p. 150.

sweated him : in that of 110° , the perspiration was more profuse, and streams of water ran down his body : in ten minutes more, he entered the room of 120° , and after staying there twenty minutes, the mercury of the thermometer which he held in his hand, and placed under his tongue, stood just at 100° , and his urine was of the same temperature. His pulse was 145, the veins on the surface of the body were much enlarged, the external circulation greatly increased, and an universal redness and strong sense of heat diffused itself over the whole body. He afterwards remained fifteen minutes in a room heated to 139° , and the temperature of his body under the tongue, in his hand, and urine, did not exceed 100° . Sir Joseph Banks, Sir Charles Blagden, and Dr Solander, went afterwards into rooms heated to 212° , the heat of boiling water, where they remained several minutes. The feeling communicated by the air was unpleasant, but easily borne : and respiration was little affected, except in the want of that refreshing coolness which the inspiration of cool air imparts. If they breathed on the thermometer, it sank several degrees ; and every expiration was cool to the nostrils, previously scorched by the hot inspired air. The body, to the touch of the fingers, felt cold as a corpse, and the actual heat of the skin, and under the tongue, was 98° . Sir Joseph Banks alone sweated profusely. In a subsequent experiment, Sir Charles Blagden remained eight minutes in a room heated to 260° : the air felt hot, but did not give pain, and the sweating was not profuse : the sensible heat of the body varied but little. For seven minutes, the breathing

continued perfectly natural, but anxiety and oppression then came on : the pulse was 144. When the clothes were stripped off, the air at 212° was more disagreeable for five or six minutes, until a profuse perspiration breaking out, gave instant ease : the breathing during this experiment was not oppressed, partly because the pulse was eight beats less in a minute, and partly because the experiment was made with an empty stomach, and the former with a full one *. Dr Dobson also went into a stove heated to 224° , and felt no oppressive sensation of heat, although every metal about him became speedily hot : a porter remained twenty minutes in the stove when heated to 210° , and, on leaving it, the pulse beat 164 in a minute, and the animal temperature rose only to 101.5° †. M. Tillet has observed, that girls accustomed to attend an oven, have borne, for ten minutes, a heat equal to 280° Fahrenheit ‡.

187. Other animals, when exposed to intense heats, exhibit similar phenomena. A bitch, of a moderate size, was put into a room heated to 220° ; in ten minutes, she panted and held out her tongue, but shewed no other sign of distress. After remaining in the room half an hour, when the heat had risen to 236° , the basket in which she had been confined, was opened, and its bottom was found wet with saliva. The thermometer being placed in her flank, fell to 110° , only nine degrees above the natural standard heat : and when turned into the cool

* Philosophical Transactions, 1775,

† Ibid.

‡ Mem. Acad. 1764.

air, she was as brisk and lively as ever, and apparently not in the least injured by the heat. That the heat of these rooms, was truly indicated by the thermometer, was proved by the effects produced on inanimate matter ; for an egg, placed on a tin plate, was, in twenty minutes, roasted quite hard : in thirty-three minutes, a beef-steak was roasted dry, and when the heated air was directed in a stream through a bellows, it was roasted in about thirteen minutes *. On the other hand, the human body, and that of other animals, will bear great degrees of cold, without any actual diminution of its ordinary standard heat ; and thus the temperature of man is the same nearly in winter as in summer—in an inhabitant of the frigid as in one of the torrid zone.

188. Since, then, in every climate, the human body, and that of the superior animals, support a standard temperature, which varies but little under every vicissitude of heat and cold, consistent with the due performance of the animal functions, there must exist in man, and in other animals, appropriate organs by which this temperature can be at all times sustained : but no living powers of the animal system appear to be sufficient for this purpose, independent of the concurring aid of external agents. No supposed attrition between the contiguous soft parts of the animal ; no friction between the vessels and the globules of the blood ; no action of the solid parts upon one another ; no circumstances arising

* Philosophical Transactions, loc. cit.

out of digestion or fermentation in the living body ; no imagined combustion of phosphorus in the blood ; nor liberation of the phlogistic, or any other principle, through the system, can be received as sufficient to account for the uniform height and steadiness of this temperature. As, therefore, the animal system, by virtue of its own powers, is unable within itself to produce this high degree of heat, to what external agent shall we have recourse, and to what organs shall we refer the production of that elevated temperature, which, in all animals, we have seen to have place?

189. In our review of the temperature of the inferior animals, it has been observed, that insects, worms, and fishes, which have no respiratory structure similar to that of the lungs ; and the amphibia, the surface of whose lungs, in proportion to that of the body, is comparatively small, and whose blood, at each circulation, is but partially exposed to the influence of the air, possess a degree of heat but little above that of the medium in which they live : while the mammalia class has a temperature considerably higher, and birds, whose lungs bear the largest proportion to their bodies, are the warmest of all animals. The observation of these facts led naturally to the opinion, that the temperature of animals was immediately connected with the function of the respiratory organs ; and we have endeavoured to prove, that the small excess of temperature, which not only the inferior animals, but which vegetables also possess, is actually derived from the decomposition of the air by these several classes of beings, so long as living action continues. No explanation,

however, of the mode in which the air contributes to sustain animal heat, was attempted, till after the great discovery of latent heat by Dr Black. That excellent philosopher having already proved, that the change effected in the air by respiration, consisted in the formation of carbonic acid (116.), similar to what happens in many examples of combustion, ascribed the production of animal heat to the decomposition of the air in the lungs, by which its latent heat was rendered sensible, in the same manner as it is given out in combustion. The blood, in its passage through the lungs, had, he conceived, its temperature by this means raised; and thus was rendered capable of communicating heat to all parts of the body, in the course of its circulation through the system. To this it was objected by Dr Cullen, that, if true, the temperature of the body ought to be greatest in the lungs, and to diminish gradually as the distance from the lungs increases, which is not according to fact. This difficulty was removed by the ingenuity of Dr Crawford, who, by a happy extension of Dr Black's doctrine, maintained, that the heat, liberated by the decomposition of the air in the lungs, passed into the blood, and existed in that fluid in the form of latent, or, what is now termed, specific heat, in consequence of which its temperature was not raised; and that this heat, by other chemical changes, was given out by the blood in a sensible form during its circulation.

190. In what manner, then, does the air, breathed by the superior animals, give out its heat, to support that high degree of temperature above the surrounding medium, which they all possess? We

have seen reason to conclude, that the inspired air is decomposed in the bronchial cells (114.) of the lungs, and that all its oxygenous portion which disappears, is converted (123.) into carbonic acid, by carbon emitted from the exhalent surface (157.) of those organs. During this gradual conversion of the oxygen gas, a quantity of specific caloric (166.), much greater than what is necessary to maintain the elasticity of the carbonic acid that is formed, is necessarily set free; and to this excess of heat, thus constantly liberated in the lungs, by the decomposition of the air, do we look as the source of that superiority of temperature, above the surrounding medium, which man and other animals, under every vicissitude of climate, are enabled to exhibit and maintain.

191. But if a quantity of caloric be thus constantly disengaged in the lungs, it may be expected that the blood, in its transmission through those organs, should acquire a certain portion of it. To ascertain this point, Dr Crawford, pursuing the discoveries of Drs Black and Irvine, mixed together certain quantities of water, at the temperature of 53° , with separate portions of arterial and venal blood; and then measuring the heat of the mixture, at different successive periods, till coagulation took place, he found, that the water containing arterial blood preserved a superiority of temperature over that mixed with venal blood; and, from the results of several trials, he concluded, that the specific heat of the arterial blood of a dog, was to that of the venal as 114 to 100, and that of a sheep as 115

to 100, or as $11\frac{1}{2}$ to 10 *. These results derive confirmation from the experiments of Mr Coleman, who, in order to discover the relative specific heat of arterial and venal blood, while yet retained in the system, strangled a cat, and immediately opened its chest, while the blood in the left ventricle was still florid. He then introduced a thermometer, through an opening in the pericardium on each side of the heart, and it stood at 98° : in the left ventricle, the temperature was only 97° , and in the right ventricle it was nearly 99° . In fifteen minutes, however, instead of the right ventricle possessing two degrees of heat more than the left, it was found to have four degrees less. Mr Astley Cooper repeated this experiment in different ways, and found invariably, that although the venal blood was superior in temperature at first, yet before coagulation was complete, the arterial became from three to six degrees warmer †. These facts afford clear and decisive proof, that the specific heat of the arterial blood exceeds that of the venal, and demonstrate, likewise, that this excess is obtained during the passage of that fluid through the lungs.

192. Admitting the lungs, then, to be the organs in which, by a decomposition of the air, the blood, as it passes through them, obtains its heat, it is next required to shew the sufficiency of this decomposition, to supply heat enough for the maintenance of that superiority of temperature, which the warmer

* On Animal Heat, p. 279.

† Coleman on Suspended Respiration, p. 42. et seq.

blooded animals possess. It is now very generally allowed, that the caloric given out during the combustion of bodies, is, like that obtained in respiration, derived from the changes which the oxygen gas of the air is made to undergo. When, therefore, equal quantities of oxygen gas are converted into carbonic acid by combustion and by respiration, equal quantities of caloric may be expected to be set free; and this opinion, too, Dr Crawford attempted by experiment to establish. He found, that 100 ounce measures of oxygen gas, changed into carbonic acid by the respiration of a guinea-pig, shut up in a close vessel, communicated to 31 lb. 7 oz. troy of water, surrounding that vessel, 17.3° of heat; while the same quantity of oxygen gas, converted into the same acid by the burning of charcoal, communicated to a like quantity of water 19.3° of heat; and he concluded, therefore, that somewhat more heat was produced by the combustion of charcoal, than by the respiration of an animal*. M. Lavoisier, likewise, made experiments of a similar nature, but he substituted the melting of ice, as a measure of the comparative heat given out by respiration and combustion, for the raising of the temperature of water; and the conclusion which he drew, when equal quantities of oxygen gas were changed into carbonic acid by the respiration of a guinea-pig and the combustion of charcoal, made the quantity of heat produced by the animal, exceed that by the combustion of charcoal in the propor-

* On Animal Heat, p. 351.

tion of 13 to 10.3 *. Although, therefore, the results, obtained by these philosophers, do not completely coincide, yet in experiments of so much difficulty and delicacy, the small difference that exists is not sufficient to invalidate the general conclusion, That, “when equal quantities of oxygen gas are converted into carbonic acid by animal respiration, and by the combustion of carbon, nearly equal quantities of caloric are set free †.”

193. But farther, as the quantity of heat produced, when a given bulk of carbonic acid is formed by the combustion of charcoal, has been demonstrated by Lavoisier ‡; and as the quantity of air usually respired (85.) has been ascertained, it is evident, that the quantity of heat liberated in the lungs, in any given time, could also be found out, were we able to estimate the quantity of carbonic acid contained in air which has been once breathed. Thus, says Dr Menzies, if the bulk of air commonly inspired be estimated at 40 cubic inches, and the number of respirations at 18 in a minute, the volume of air inspired every minute will be 720 cubic inches; of which $\frac{22}{100}$, or 158.4 cubic inches, consist of oxygen gas, the only part of the air changed by respiration. But of the air thus inspired, $\frac{1}{20}$ only, or two cubic inches, are changed in each respiration, which gives for the amount of carbonic acid, formed every mi-

* Mém. de l'Acad. des Scien. 1783.

† Crawford on Animal Heat, p. 353.

‡ Kerr's Lavoisier's Elements of Chemistry, 4th edit. p. 113.

nute, 36 cubic inches, or 51840 cubic inches in the space of a day, equal in weight to 3.9697 lb. troy. But for every pound of carbonic acid formed by the combustion of charcoal, a quantity of heat is given out, according to Lavoisier, sufficient to melt 27.02024 lb. of ice : therefore, 3.9697 lb. of this acid, produced daily in respiration, will furnish heat enough to melt 107.2 lb. of ice ; for $27.02024 \times 3.9697 = 107.2622$. Of this quantity of heat, however, a portion, adds Dr Menzies, is carried off in the form of sensible heat with the air that is expired, and another portion is rendered latent by combining with the vapour that issues from the lungs. These two portions, from experiment, he calculates to amount to heat sufficient to melt 32.9833 lb. of ice ; which sum, subtracted from 107.2622, leaves a remainder of 74.2789 lb. as the quantity of ice capable of being dissolved by the heat daily set free, by the decomposition of the air, in the lungs of an ordinary man *. How admirably the blood in the lungs is disposed to receive this heat, the vast extent of the cellular surface (114.) of those organs, and the decomposition of the air, and consequent extrication of its heat, going on over every point of that surface, furnish abundant proof.

194. The inferences drawn from the foregoing experiments and calculations appear to be just, if the premises on which they are founded be in all respects correct : but neither the quantity of air ta-

* Menzies on Respiration, p. 50. et seq.

ken in at each inspiration, nor the portion of it that is changed, nor the number of respirations made in a minute, are yet finally agreed on; and, indeed, from various causes, they must be subject always to much variation. Greater accuracy, also, in the experiments of Crawford and Lavoisier, as to the comparative quantities of heat, produced by respiration and by combustion, is desirable; and in those of Dr Menzies, the abstraction of heat by the air and vapour expelled from the lungs, appears to be much overrated. In their present state, therefore, these inferences must be considered only as approximations to the truth; but the particulars on which they rest, seem to embrace all the necessary considerations, if in every instance they were accurately determined. At any rate, it may, we think, be safely held, that in the lungs of animals, sufficient air is decomposed to furnish a large quantity of heat; and the fact, that the temperature of all animals is in proportion to the relative capacity which their respiratory organs bear to the body, and to the quantity of air which they breathe in a given time, appears to justify the conclusion, that animal temperature is derived chiefly, if not entirely, from this source alone.

195. But we have seen, that the oxygen gas of the air is converted into carbonic acid by the human skin (147.) and intestines, as well as by the lungs; and in many of the inferior animals, their superiority of temperature is sustained by the changes induced on the air through the agency of the skin alone. As, therefore, the skin, by the same powers, changes the air after the same manner as the lungs, it is reasonable to infer, that phenomena similar in kind,

result therefrom; and that as insects and worms, by means of their skins, preserve a superiority of temperature over the medium in which they live, so likewise those surfaces of the human body which communicate directly with the air, derive from this source a degree of heat also, by which means they become, in some measure, auxiliary to the lungs*.

196. Besides this production of heat, as the principal phenomenon arising from the change which the air suffers in respiration, it has been supposed, that water also is formed by an union of a portion of the inspired oxygen gas with hydrogen residing in the blood. But Mr Davy remarks, that there are no reasons for supposing any residual atmospheric oxygen to be immediately combined with fixed or nascent hydrogen, or hydro-carbonate, in the venal blood at 98° ; and, consequently, none for believing that water is immediately formed in respiration†; and Dr Bostock observes, that the discharge of hydrogen from the blood has been admitted with-

* This power in the skin to produce heat, naturally occurred to the author from the view which he had taken of the effects produced on the air by that organ. From information which he has since obtained, he learns, that the same opinion was, many years ago, taught by Mr Allen in the lectures on physiology which he delivered in Edinburgh; and a similar doctrine has been pointed out to him in the "System of Chemical Knowledge" by M. Fourcroy, where, however, it is obscured by the language of that imperfect analogy which chemical physiologists never fail to institute between respiration and combustion.

† Researches, p. 423.

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out sufficient evidence *, and conceives, with Mr Davy, that the pulmonary exhalation proceeds from a secretion carried on by the vessels of the lungs. Moreover, we have endeavoured to shew, that no oxygen gas can enter into the blood-vessels, but that the whole of it that disappears is decomposed in the lungs, and is actually expended in forming the carbonic acid (123.) which is there produced : so that none remains for the formation of water by uniting with the supposed hydrogen of the blood. Even if oxygen gas did enter into the blood, and hydrogen also existed in that fluid, we have no proof that their affinity for each other is sufficient to form water ; for, out of the body, actual ignition, or a great degree of compression, is required to reduce them to a fluid form.

197. If we consider the vast extent of the cellular surface (114.) of the lungs, and the similarity which in its structure and functions (157.) it bears to that of the external surface of the body, we are naturally led to the belief, that the pulmonary excretion, like every other, and especially like that from the skin, is carried on by an appropriate structure, and according to the ordinary laws of the exhalent system. That the great bulk of fluid exhaled from the lungs is derived from this source, none will venture to deny. Why then should we imagine another mode of production for this fluid, which is so totally at variance with it? At least,

* On Respiration, p. 129.

the insufficiency of the ordinary function to furnish all the water exhaled, ought first to be clearly proved. The daily loss of weight which the whole body experiences by the escape of perspirable matter, is estimated by Haller at about 60 oz. in the warmer climates, and from 56 to 30 in more temperate climes *. Of this latter quantity, Dr Hales calculated the loss by the pulmonary exhalation to be about 6 oz. †; but the more accurate trials of Lavoisier and Seguin make it to amount to about one-third of the whole. The mean loss sustained by perspiration was, according to them, 2 lb. 13 oz., of which the pulmonary discharge was 15 oz. and the cutaneous 1 lb. 14 oz. ‡. When, therefore, we bear in mind, that the surface of the bronchial cells is ten times greater (114.) than that of the external skin, and is, like it, duly furnished with exhalent vessels, we are so far from seeing the necessity of resorting to the supposition that water is formed chemically in the lungs, that we can more readily imagine the pulmonary exhalation to be rated in these estimates below what the extent of its exhalent surface may be considered able to supply.

198. Rejecting, therefore, the opinion, that any water is formed by the chemical union of oxygen and hydrogen in the lungs or blood-vessels, it is not within our plan (which professes to treat only

* *Elementa Physiol.* tom. v. p. 58.

† *Vegetable Statics*, vol. i. p. 10.

‡ *Mem. de l'Acad.* 1790.

of the phenomena which arise out of the changes the air suffers) to inquire farther into the laws of its production. One consequence, however, which seems to follow from considering the aqueous vapour to be wholly emitted by the exhalents of the lungs, we cannot omit to notice; for, on this supposition, there seems no reason to think that any part of the caloric liberated in the lungs by the decomposition of the air, is, as Dr Menzies imagined, employed to raise that fluid into vapour. The power of the skin, when its heat is many degrees below that of the lungs, is sufficient to emit its excretion in a vaporific form, nor is there any reason why the exhalents of the lungs should not, without the aid of a fresh portion of caloric, emit vapour also. In fact, both the external and internal surfaces of the body always do so; and it is only when the exhalation is excessive that the insensible perspiration is condensed into a fluid form.

HERE, for the present, the author closes his inquiry. After the review which has now been taken of the changes induced by living vegetables and animals on the air, it was originally his intention to have entered on an investigation of the effects which these changes produce upon them. Had he pursued the subject, it was his design, by

a strict application of the preceding facts, to have attempted the illustration of certain phenomena which occur in germination and vegetation ; and likewise in the evolution, continuance, and suspension of living action in the inferior classes of animals. The phenomena of incubation, and the evolution of viviparous animals, would next have engaged his attention ; and these would have been succeeded by an endeavour to explain the phenomena of respiration, as connected with the appearances and properties of the blood, and the distribution and maintenance of animal heat. A short view of some pathological states of the system, arising immediately out of the preceding discussions, would then have concluded the whole.

This enumeration, it is obvious, comprehends some of the most curious and important problems in physiology ; and many of them are of such a nature as not to admit, in the present state of science, of completely satisfactory proof. All, therefore, that could be hoped for, would be a probable explanation, essentially different, indeed, from all the theories which have been hitherto proposed, but perfectly consistent with the view already taken, and supported and illustrated by the aid of such additional facts and experiments as a farther prosecution of the subject would necessarily introduce.

As, however, the whole of the author's reasoning would be founded on the principles which, in this publication, he has attempted to establish, it

is evident that the truth or falsity of the principles themselves, ought first to be ascertained. With this view, he submits them to the judgment of the public in their present form. If they shall be favourably received, and the author be in consequence encouraged to proceed, he will endeavour to execute the remaining part of his plan in the best manner that he is able. If, on the contrary, they shall be shewn to be fallacious, he will at once desist from the farther prosecution of the inquiry: and will thus be spared the mortification of adding to the perplexity of a subject which he was unable to explain.

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